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THE JOURNAL

OF

THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

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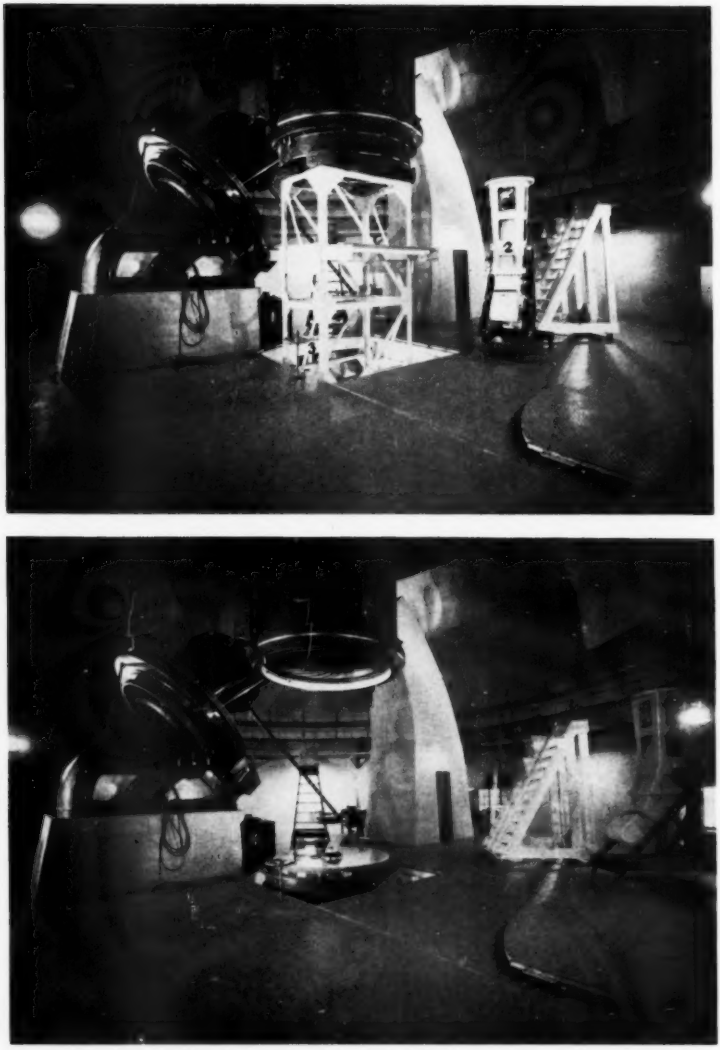
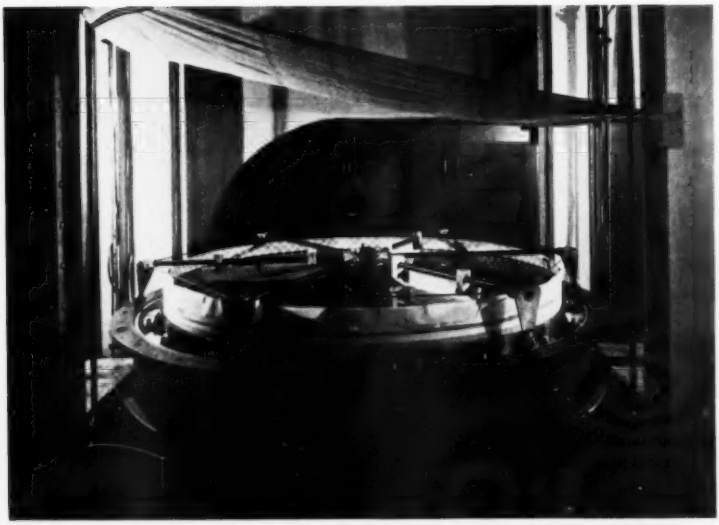
PLATE I.

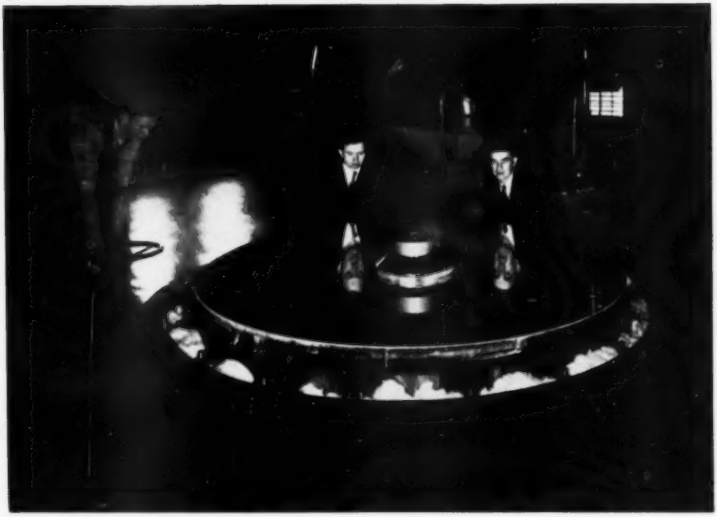


(a) *above* The elevator raised, preparatory to unbolting the 74-inch mirror from the telescope tube.

(b) *below* The 74-inch mirror being lowered into the silvering room.

PLATE II.





(a) *above* The mirror in the silvering room showing the webwork for stirring the silvering solution in position.

(b) *below* The freshly silvered mirror on its way up from the silvering room.

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SILVERING THE 74-INCH MIRROR

By R. K. YOUNG

(With plates I, II)

PLATES I and II show four pictures taken during the process of silvering the main mirror of the 74-inch telescope of the David Dunlap Observatory. It has always been our intention and hope to obtain facilities for aluminizing the surface of this mirror. An aluminum coating is much more permanent and superior in reflecting power in the violet region of the spectrum. The secondary mirrors were coated with aluminum for us through the courtesy of the Mount Wilson Observatory in 1930 and are still in fairly good condition. We have found it necessary to silver the main mirror two to three times a year.

As many of the readers of this JOURNAL have had experience in silvering their own mirrors, they may be interested in the process and difficulties involved in silvering the large mirror.

It is necessary first of all to get the mirror off the telescope and into a room which can be heated. The dome itself being without heat and the silvering process requiring a temperature at least above 40° F., the process becomes impossible in the winter unless this can be done. The mirror is carried in a large cast-iron cell which, together with the mirror, weighs approximately 4 tons. The cell and mirror are taken off the telescope in one unit. To do this the telescope is pointed to the zenith and first of all securely anchored. This is done by lashing the upper end of the tube to the dome and by placing a brace rod against the declination axis to take the thrust due to lack of balance in right ascension when the mirror and cell are removed. This brace rod may be seen in Plate I (a).

The carriage to hold the spectrograph is then placed in position and the spectrograph removed and wheeled to one side (2). A cover plate in the floor is next raised and pushed to one side and a counterbalanced elevator is raised by a hand wheel shown at (3) until it makes contact with the mirror cell as shown in the plate. The counter-weights for this elevator consist of cast-iron slabs weighing about 600 lbs. each. They are strung on long threaded eye bolts and when the elevator is at the top these counterweights go to the bottom. Extensions in the eye bolts then pass through extra counterweights which are then attached by nuts. The extra counterweights are just sufficient to counterbalance the elevator plus the mirror and mirror cell. Twenty-four bolts around the mirror cell are then removed and the mirror cell is free from the telescope. It can be readily lowered by hand. In Plate I (b) it is shown at the floor level. In Plate II (a) it is shown at rest in the silvering room. A canvas is stretched about 3 feet above the surface to prevent dust from dropping on it and to protect it from any chance accident from above.

In Plate II (a) there is shown a wall of oilcloth about six inches wide stretched around the mirror and held in position by large elastic bands formed by cutting an automobile tube. Also there is shown a webwork of six tubes which carry wooden fins almost in contact with the surface. This webwork can be rotated, running on rollers around the edge of the cell, and the purpose of this is to stir the silvering solutions on the mirror.

The surface of the mirror is thoroughly cleaned before any of these attachments are put in place. The old coat of silver is first dissolved off with concentrated nitric acid and if thought necessary (if the surface appears at all greasy) with caustic potash. It is then washed with ordinary tap water and given a thorough cleaning with soap and water. We have found that the most suitable material for rubbing the surface of the mirror is a soft grade of flannelette. This does not seem to scratch and does not leave any stray lint which is so liable to be troublesome if absorbent cotton is used. The oilcloth band is next put in position and the soap well rinsed off with plenty of tap water and well swabbed. After this it is rinsed in distilled water and sufficient distilled water placed on the mirror to fill the

concavity. This takes about ten gallons. The stirring device is then placed in position and the mirror is ready to receive the silvering solutions.

We have found the oilcloth to be superior to a waxed paper as a retaining wall because a certain amount of the wax seems to get into the solution near the edges and interfere with the silvering process for the space of a half inch or inch around the outside.

If the coat is successful the old solution is allowed to drain quickly through the hole in the centre of the mirror, the solution being carried by a pipe to a surface drain, and any sediment rinsed off with tap and then distilled water. We have found the Electrolux vacuum cleaner very convenient in drying off the surface as quickly as possible. It is usually necessary to burnish the surface lightly after it is dry and hard. Plate II (b) shows the freshly silvered surface. The picture was taken as the mirror was passing upward through the floor on its way to be reattached to the telescope. The distortion of objects in its surface can be readily observed. The whole process takes about a day and it is quite possible to take the mirror off, silver it, and return ready for work the same night, though as yet, due to a delay of one kind or another we have usually allowed the mirror to harden for a time before burnishing and not put it back in place till the next day.

TELESCOPE MOUNTINGS FOR AMATEUR BUILDERS

By H. Boyd Brydon

(*Continued from December issue page 391*)

IV

Some other forms of the equatorial mounting may now be considered.

With the mountings so far described certain inconveniences arise when the aperture of the telescope is much over 6 inches, owing to the position of the eyepiece. Being at the top of the tube it cannot be reached from the ground and some sort of staging must be used to stand on. The eyepiece moves over a considerable area, nearly a hemisphere, and in parts of its travel assumes positions which are awkward, fatiguing, and may lead to accident. Also it becomes increasingly difficult if not impossible, as one grows older, to withstand the numbing cold of a winter night. If any observing is to be done during that season an observing room at a comfortable temperature becomes a necessity.

Were the focal plane of the telescope made stationary, not only would these difficulties be avoided but the operation of cameras, spectroscopes, etc., would be much easier. Now by using a siderostat, an equatorially mounted plane mirror, the light from a star can be reflected in a constant direction—along the polar axis of the siderostat for instance. It can then be received by an objective and brought to a focus at a stationary point. Similarly, a parabolic mirror can be placed in that axis to reflect the light down the axis to a diagonal mirror and, as the focal image of the star is stationary, a stationary eyepiece can be used.

Several important refracting telescopes have been built on this plan. Among them are the well known coudé telescopes at the Paris observatory, the 12½-in. Sheepshanks telescope at Cambridge, and the Gerrish at Harvard. The snow and tower telescopes at Mt. Wilson are other examples.[9] Fig. 7 shows a Newtonian reflector on this plan, designed and built by Russell W. Porter for his observatory at Port Clyde, Maine.[10]

[9]Dr. Louis Bell illustrates several of these instruments in his book "The Telescope".

[10]R. W. Porter, "A Polar Reflecting Telescope", *Pop. Ast.*, vol. 24, p. 308, 1916. The adaptation of this principle to the Cassegrain is also described in this article.

The additional reflecting surface, the feature of all these designs, does enable the use of a stationary eyepiece at which the observer can sit in a comfortable observing room with maps, note books, etc., conveniently at hand, but some rather serious disadvantages are involved.

1. The siderostat mirror, which must be at least of equal diameter to the main mirror of the telescope, adds an optical

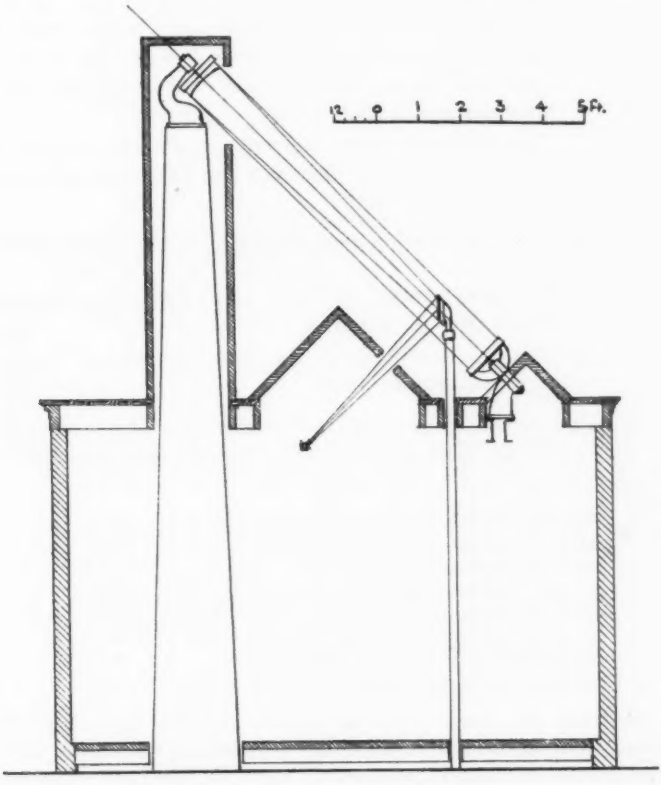

Fig. 7. Porter Polar Reflector.

surface where any defect of figure most affects definition. To figure, say, a 12-in. or better a 12-in. by 18-in. mirror flat to one quarter of a wave length or thereabouts is a formidable task.

2. Loss of light occurs from

   (*a*) The additional reflection.

   (*b*) Reduction of the effective area of the parabolic mirror as the polar distance of the axis of the siderostat mirror increases. By using, as suggested above, an oval siderostat mirror having a major axis greater than the diameter of the parabolic mirror, loss of light from this cause can be reduced to some extent, but the end is soon reached in this direction.

   (*c*) The greater diameter of the diagonal mirror required by the greater distance between the axis of the telescope and the focal plane.

3. The arc in declination available for observation is restricted by:

   (*a*) The area around the pole hidden by the parabolic mirror and its mounting.

   (*b*) The practical limit to the polar distance of the axis of the siderostat mirror.

4. Absence of any practical way of fitting a finder telescope.

The following dimensions of Mr. Porter's telescope may be of interest:

Siderostat mirror, diameter	16 in.
Parabolic mirror, diameter	16 in.
Parabolic mirror, focal length	15½ ft.
Diagonal mirror, minor axis	7 in.
Diagonal mirror, major axis	10 in.
Diagonal mirror to focal plane about	3 ft. 10 in.
Thickness of siderostat and parabolic mirrors	2 in.
Thickness of diagonal mirror	1 in.

Perhaps the most effective attack on the difficulties besetting the problem of combining a reflecting telescope and a comfortable observing room is due to Mr. P. R. Allen of Auburndale, Mass. Mr. Allen has adopted the expedient of expanding the polar

axis into a ring lying in the plane of the equator and within which the observer stands when at work.[11] Fig. 8 shows a general arrangement of this design. The equatorial ring rotates on steel balls on a bearing ring supported by the walls of the observatory and forms the base to which the roof of the so-called turret is attached.

At a point opposite the diagonal mirror the telescope is attached to a tubular declination axis turning in bearings fixed to one end

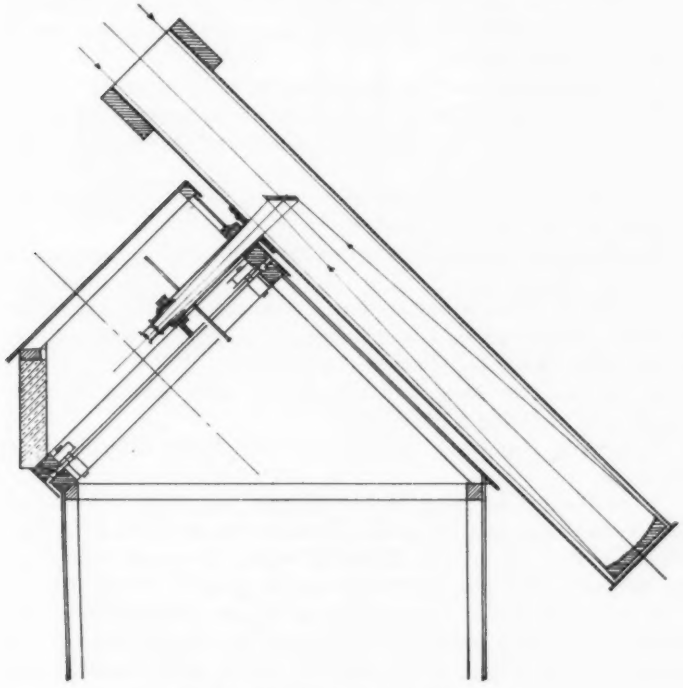

Fig. 8. The Allen Turret Mounting.

[11]P. R. Allen, A Turret Reflector, *Pop. Ast.*, vol. 35, p. 384, 1927. This development of the ring type of polar axis was originated by Mr. James Hartness. It forms the basis of his turret refractor described in a paper presented in 1911 to the American Society of Mechanical Engineers.

of a diameter of the ring. Counterbalance in right ascension is obtained by weighting the opposite side of the ring and in declination by a weighted extension of the telescope tube.

By this construction, not only is no siderostat required but a Newtonian reflector of standard optical design is used, except that, being farther from the focal plane, the area of the diagonal mirror must be about 8½ per cent. of that of the principal mirror instead of the 2½ to 3 per cent. usual for a focal ratio of F8, but the resulting loss of light is much less than would be occasioned by the use of a third reflecting surface.

The disadvantages of the design may be briefly stated:

1. It is rather difficult to place the rings accurately in the equatorial plane and maintain that adjustment against any movement of the building.

2. Vibration of the exposed telescope by wind is to be expected and will be difficult to control.

3. The telescope must be reversed when observing across the meridian.

4. The eyepiece is not stationary but moves in an arc of the equator. The observer must change his position correspondingly in the rather restricted space within the ring.

5. As in other designs of this nature there seems to be no simple way of fitting a finder.

The most important of these are the finder difficulty and vibration.

Notwithstanding these disadvantages, Mr. Allen's mounting appears to remove in a very practical manner many of the difficulties attending the comfortable observing room. It would seem that the difficulty of fitting a finder is not so great as would appear.

If the comfortable observing room be not insisted upon, the stationary eyepiece condition can be met in a simple manner. A modified German mounting is used in which the declination and polar axes are hollow. The diagonal mirror is turned to direct the rays through the declination axis to its intersection with the polar axis, where a second diagonal mirror, or better, a totally reflecting prism, turns the rays polewards to the eyepiece inserted in the upper end of the polar axis. Thus the observer does not have to move around when using the telescope but sits looking down the polar

axis. A mounting of this type has become widely known and used under the name "Springfield". A vertical section taken through the declination axis is shown in Fig. 9.

So unconventional a design naturally has received its full share of criticism. Much is merely conservative dislike of the bold departure from established practice but some may not be un-

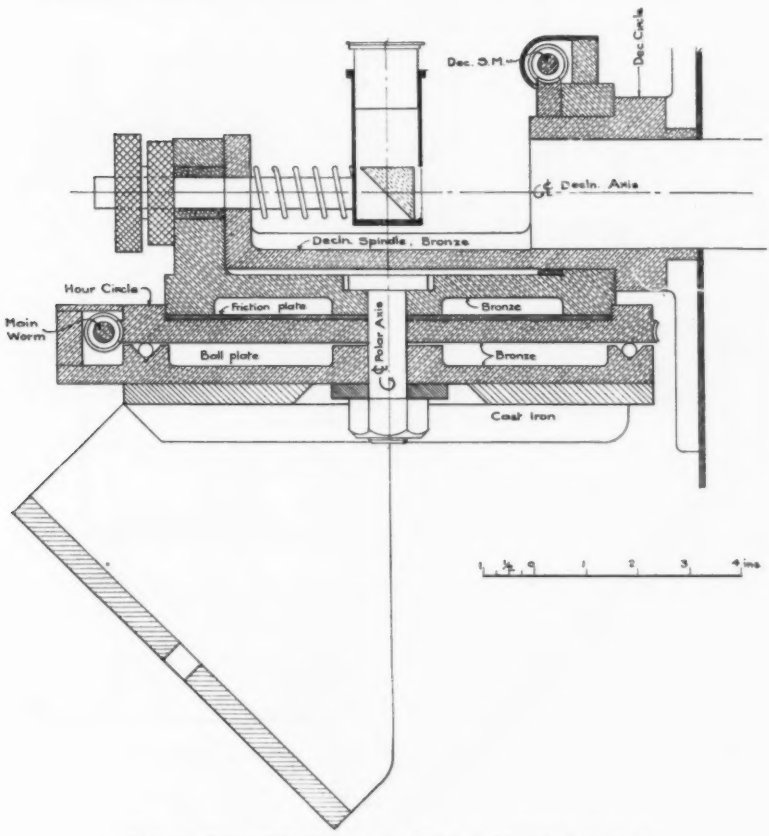

Fig. 9. Springfield Stationary Eyepiece Mounting Type 5.

warranted. For instance, as the whole weight of the telescope swings by its eyepiece end on a not-too-rugged declination axis, some deflection of the optical axis may take place. There is little doubt that the shape of the declination axis, which is of bronze, is such as to introduce a structural weakness at the point where the bending stresses from the telescope and its counterpoise are concentrated, and this condition is not helped by the short pin and

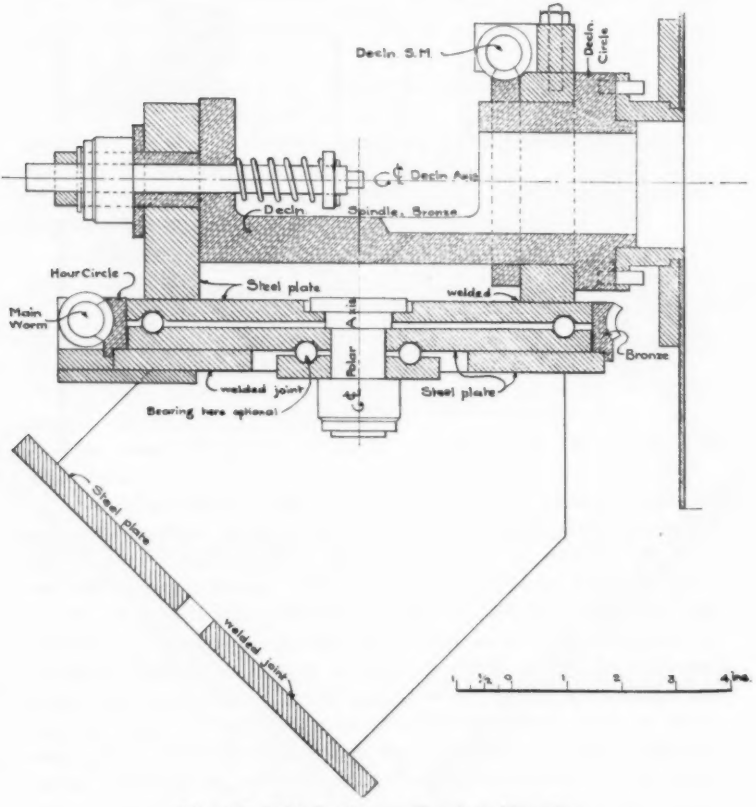

Fig. 10. Reid Stationary Eyepiece Mounting.

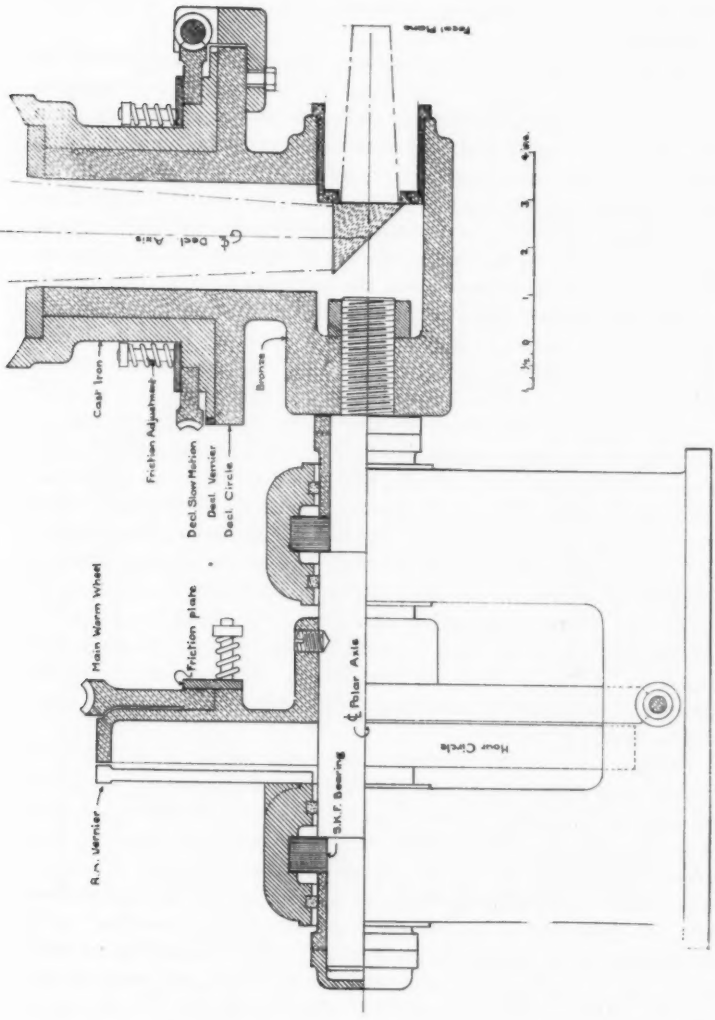

Fig. 11. Pasadena Stationary Eyepiece Mounting.

thin bronze disc forming the polar axis, which is itself rather liable to flexure.

In general the mounting seems unnecessarily complicated in construction, involving a great deal of pattern and fine machine work. Extravagant use is made of bronze castings; even the main bearing of the polar axis disc which supports the whole moving load has bronze ball races and bronze balls, hardly in accord with usual engineering practice and must add to the cost of the mounting.

A simpler mounting on the same general lines has been designed and built by Mr. H. A. Reid, a member of this Society, of Coleman, Alberta. By his kind permission the following notes are presented.

Fig. 10 shows a section through this mounting taken on the centre line of the declination axis. By comparing this with Fig. 9 the principal differences will be evident. By economical design and by taking full advantage of the ease of getting electric-welding done nowadays steel plates or steel bars have been substituted for many of the bronze castings while others have been eliminated. The cast iron base of the Springfield has been replaced by a base built up of $\frac{3}{8}$-in. steel plates bolted together and welded. Only three bronze castings are used: the ring for the main worm wheel; the declination spindle; and a collar on the declination spindle, the edge of which, as in the prototype, serves as a rather small declination circle. Altogether Mr. Reid's mounting is a straightforward simple job on good engineering lines, easier and much less expensive to build than its forerunner. The almost complete absence of castings will commend it to those who are distant from large cities.

Another stationary eyepiece mounting which is not so well known as it deserves to be is the "Pasadena" mounting, designed by Mr. F. M. Hicks of that city, with the advice and assistance of the Engineering Department of Mt. Wilson observatory. The substantial and clean-cut construction of this mounting will be at once evident from Fig. 11, which is a section through the declination axis. A good deal of bronze casting and precision machine work is required in the construction of the Pasadena mounting so that it cannot be considered as an inexpensive design, but many of the possibly objectionable features of other mountings of the fixed eyepiece type are absent and the details have been thoroughly

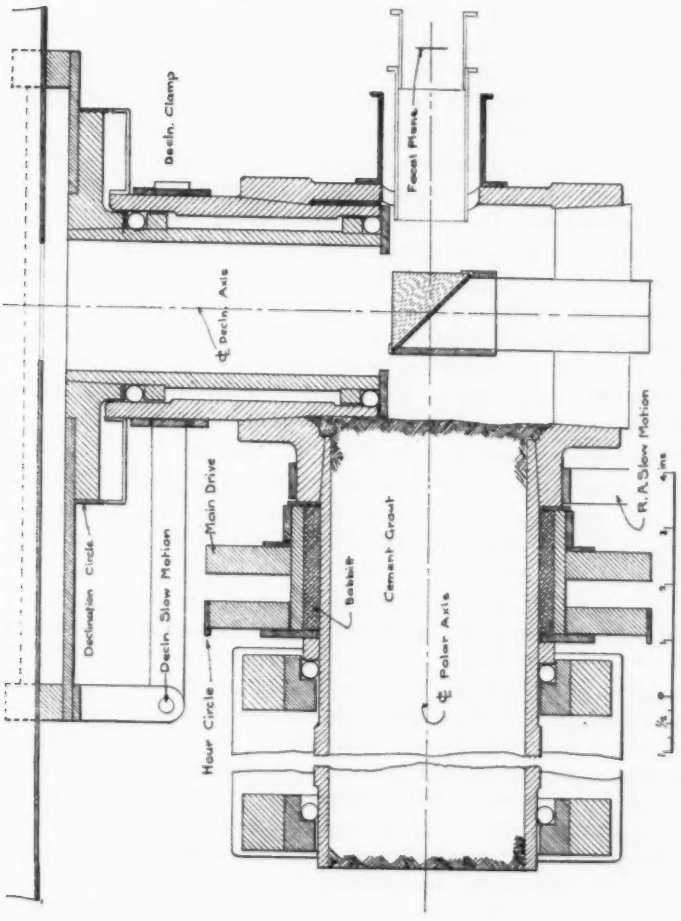

Fig. 12. Fixed Eyepiece Mounting.

worked out, the use of a standard form of polar axis, carried in S.K.F. ball bearings being a case in point.

An inexpensive fixed eyepiece mounting can be made quite satisfactorily using pipe fittings. A design suitable for a telescope weighing up to about 35 lbs. is shown in Fig. 12. The telescope is carried in a cradle of steel plate bolted to a standard 2½-in. pipe flange, reduced in thickness to save weight, and screwed onto a short piece of 2½-in. extra-strong black pipe which forms the declination axis. The declination axis housing is a piece of 3½-in. extra-strong black pipe threaded on one end so that it will reach as shown into a 3½-in. standard tee beyond the usual distance. These pipes are turned and bored on the ends for ball bearings. The polar axis is of 3½-in. standard weight black pipe filled with cement grout as in the mounting shown in Fig. 5. The space within the tee surrounding the inward projection of the declination housing is also thoroughly packed with cement grout.

The drive wheel, R.A. slow motion, and the free hour circle are carried on a sleeve on the polar axis which runs in ball bearings.

The second diagonal mirror is inserted through a standard plug in the free end of the tee which allows for adjustment when erecting.[12]

To avoid the second diagonal mirror of the mountings just discussed Professor W. H. Pickering has suggested[13] a modified fork mounting in which also, by placing the eyepiece in either of the pivots forming the declination axis, its objectionably wide movement, while not stopped, would be reduced to the comparatively negligible motion of the pivot. Being carried between pivots the telescope should be steadier than with the single bearing and the large overhang of the mirror, some seven diameters for an F8 focal ratio, instead of being counterbalanced by a ball on a long iron rod, would be easily counterbalanced by a weighted extension of the telescope tube. To enable the eyepiece to be used in either pivot the diagonal mirror would be carried in a rotatable inner tube.

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[12]Complete detail drawings of these mountings may be obtained.

[13]W. H. Pickering, "The Practical Use of Small Reflectors", *Pop. Ast.*, vol. 34, p. 573, 1926.

The proposed mounting is shown in Fig. 13. In a later article[14] Professor Pickering says of this mounting: "As in all mechanical devices where considerable advantages are secured, there must be some sacrifice. The chief one is that the tube must be about half as long again as in the ordinary form of Newtonian in order that the single counterpoise may not be more than twice as heavy as the mirror" (plus its cell and part of the tube). "The second disadvantage is that there is a region surrounding the north pole with a radius of at least 20° and perhaps 30° which cannot be observed

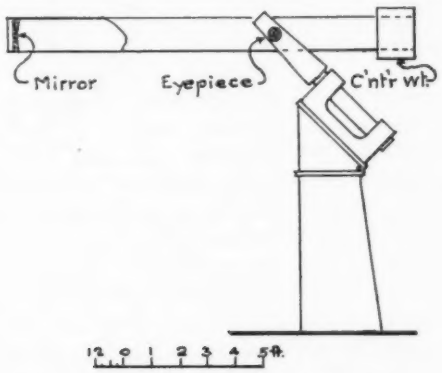


Fig. 13. Pickering's Proposed Fork Mounting.

at all. . . .This obviously does not bother planetary observers" (among whom is Professor Pickering). It is also stated that, having a fork mounting, "the telescope never has to be reversed". This is an oversight for it will be clear from Fig. 13 that this applies only when observing objects from the zenith southwards.

The proposed design as it stands involves perhaps more of sacrifice than of benefit but it is interesting as representing the limit of its type; a fork mounting with nearly stationary eyepiece. A compromise between this and the standard fork mounting might

[14]W. H. Pickering, "Reflectors versus Refractors", *Pop. Ast.*, vol. 38, p. 134, 1930. These two articles by Professor Pickering contain valuable matter and merit careful reading.

be found useful in some cases, but for a telescope counterbalanced as described by Professor Pickering the Bailey modification of the yoke mounting undoubtedly would prove more satisfactory.

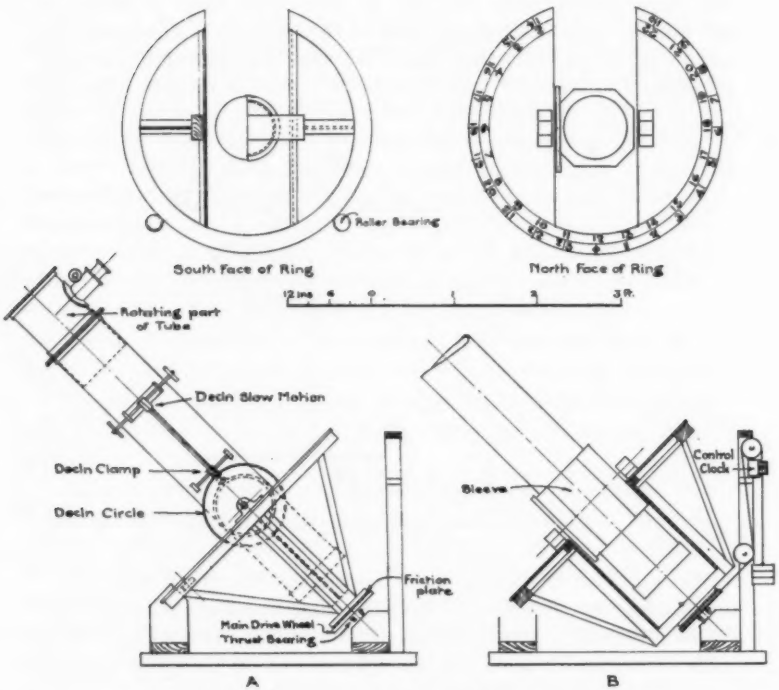


Fig. 14. Jorgenson's 8-in. Wooden Mounting.

A Wooden Mounting

By the kindness of Mr. Carl Jorgensen of Vancouver Centre I am enabled to present a mounting of a very different but none-the-less successful kind. In sending me the sketches from which Fig. 14

was prepared Mr. Jorgensen makes the following useful comments:

Mountings that can be made with saw and hammer are what the amateur wants; they are all the tools I have. For my 8-in. F8 reflector I have made something along the lines of Porter's "Ring" and also a double yoke mount—both out of 2 x 4's and 3-ply. Both work very satisfactorily. The ring type is rather heavy for taking indoors but is very steady in operation.

By the way, if amateurs could come to understand that "dew does not *fall*, but rises from the ground" they would build a housing and permanent mounting for their telescope.

As to drive mechanisms it is simple enough to rig up a drive with weights and use the winding key shaft of an alarm clock as a governor. An alarm clock will stand six to eight pounds of weight. I have made exposures of forty minutes with a camera with hardly a discernible movement, but the photographic plate tells us that the telescope has moved in innumerable little jerks so that this drive is not very satisfactory for photography.

The telescope is carried in a heavy wooden sleeve, see Fig. 14b, in which it can be rotated while observing to turn the eyepiece into a convenient position. A rather better plan is to arrange that the upper portion of the tube may be rotated on the lower portion. As the lower portion does not rotate, the supports for the clamps and slow motions may be attached directly to it within convenient reach when observing (see Fig. 14a). A third plan provides eyepiece holders on opposite sides of the tube and turns the diagonal mirror to suit the one in use. There is much room for question as to the relative and actual merits of such schemes. Their value seems to be largely a matter of personal preference.

V Drives

The mounting being free from avoidable and varying friction and the telescope nicely balanced both in declination and right ascension, a successful drive mechanism should move it uniformly, when set on a star, at the proper speed to compensate for the earth's rotation. In addition, the speed should be easily and simply adjustable to the relative motion of a planet or the moon. It should have ample reserve capacity to maintain the correct speed notwithstanding a varying slight lack of balance in the moving parts, as may occur, for instance, when the telescope passes from one side of the pier to the other. Gearing should be free from periodic error and back-lash. Worm shafts should be free from end-play.

In a home-made drive perhaps these things can be only approximated, but every effort to attain them should be made, for a wobbly or irregular drive is a sore trial.

Mechanically operated drives should be supplemented by hand operated slow motion devices by which minor adjustments in the setting of the telescope both in declination and in right ascension can be made without interfering with the drive mechanism. In declination, such adjustments are made by rotating the declination axis in its housing; in right ascension, by driving the polar axis indirectly. The main drive wheel, instead of being attached to the polar axis, is fixed to a sleeve which can turn freely thereon. The driving mechanism thus rotates the sleeve independently of the polar axis, which can be held stationary or turned in either direction as desired without affecting the motion of the sleeve. Then, when the telescope is approximately set in right ascension, the polar axis may be clamped to the sleeve by a connection such as is shown in Fig. 6, which permits of the polar axis being slightly advanced or retarded while clamped to the uniformly rotating sleeve.

If also, instead of securing it to the polar axis, the hour circle be mounted on this sleeve so that it may be turned freely by hand, setting in right ascension is greatly simplified as the hour circle may be set to read sidereal time at the beginning of an observing period. Then, assuming the drive to be running at the correct speed, the telescope may be set to any desired right ascension by unclamping and turning the polar axis until a pointer on it indicates on the hour circle the right ascension required. The telescope is then reclamped.

It is sometimes more convenient to use a friction plate, as in Fig. 14, instead of a clamp. While this plan avoids having to unclamp and clamp when a change of setting is made in right ascension it results in a rather undesirable stress being thrown on the drive, as the telescope is forced to the new position against the friction of the plate. Of course, if desired the same thing can be done with a clamp by screwing down lightly. In general, however, where the design of the mounting permits, the clamp seems preferable.

(*To be concluded*)

THE PHYSICAL STATE OF THE UPPER ATMOSPHERE

By B. HAURWITZ

(*Continued from December, 1936 issue, page 415*)

VIII. OZONE

Ozone has been mentioned repeatedly in this survey of the physical state of the atmosphere. The presence of a small amount of ozone in the lowest layers of the atmosphere has been determined directly by chemical methods. The first hint at the presence of ozone in higher layers and its important rôle in the heat-balance of the atmosphere was given by the abrupt end of the solar spectrum in the ultra-violet region at about 2900A. With an effective temperature of 6000°C. the sun should radiate about 61% of the energy emitted at 4000A at 3000A, and about 9% at 2000A. The termination of the solar spectrum is evidently not due to absorption in the sun's atmosphere since the shortest observed wave-length is independent of the distance from the solar limb. Neither does it seem to be due to some physical peculiarity of the sun since the same spectral limit has been found for whiter and hotter stars. On the other hand, the end of the spectrum moves from longer to shorter wave-lengths as the zenith distance of the sun or the star decreases, thus rather indicating a terrestrial origin. But the absorbing layer was soon found to be well above the troposphere because on mountains and even at 9,000 metres in balloons the limit of the spectrum was not noticeably moved to shorter wave-lengths.

It was suspected very soon that ozone was responsible for cutting off the shorter wave-lengths in the solar spectrum, because ozone has powerful absorption bands, the so-called Hartley bands, between about 2200A and 3200A with a maximum at 2550A[83]. Following these bands there are the Huggins bands around 3300A, which are not as intense and whose absorption coefficients are not so well known. In the visible region of the spectrum are the Chappuis bands, principally in the yellow and orange. Little work has been done on the bands in the infra-red[84].

Before definite proof is brought that the absorbing agent is ozone we shall use this assumption as a working hypothesis. It has been pointed out that there are reasons to believe that atmospheric ozone is in the higher levels of the atmosphere. In fig. 21 *OO*

may represent the ozone layer at an elevation h above the surface, z is the zenith distance of the sun seen from the earth's surface, y seen from the ozone layer. If the optical thickness of the layer is unity for the vertical incidence ($z=0$) it is

$$l=\sec y \quad \ldots\ldots\ldots\ldots\ldots\ldots (32)$$

and

$$\sin y=\frac{\sin z}{1+h/R} \quad \ldots\ldots\ldots\ldots\ldots\ldots (33)$$

This is correct as long as the zenith distance is not too large. For large zenith distances the secant should be replaced by Bemporad's function for atmospheric refraction.

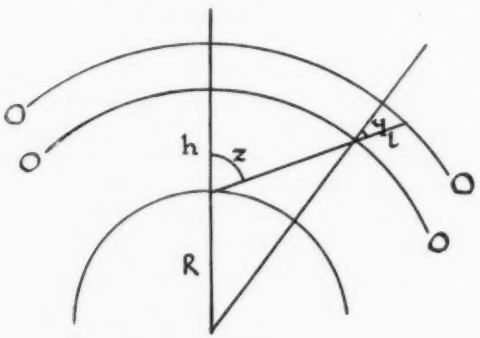


Fig. 21

The radiation intensity I measured at the surface is given by

$$\log I=\log I_0-kl \ldots\ldots\ldots\ldots\ldots\ldots (34)^*$$

where I_0 is the extra-terrestrial radiation and k the absorption coefficient. In eq. (34) I is found by observation and l is given by the sun's zenith distance, so that in principle k and I_0 can be determined by two measurements. In practice, of course, a whole series is taken and the most probable values determined. The absorption coefficient k itself is due to a number of different

*log = logarithm with base 10.

causes. There is the molecular scattering β which can be considered as known from Rayleigh's work. The quantity β is inversely proportional to the fourth power of the wave-length. Further we have the effect δ of atmospheric dust which is not so well known; and finally the total absorption by ozone which may be expressed by ax, where a is the absorption coefficient of ozone for an O_3 layer of thickness 1 cm. at N.P.T. and x the total amount of ozone expressed in cm. at N.P.T. Other absorbing agencies in the atmosphere can be eliminated if the measurements are made in suitable regions of the spectrum. Thus, k can be expressed as

$$k=\beta+\delta+ax \quad \ldots\ldots\ldots\ldots\ldots\ldots (35)$$

If by observation k is the absorption coefficient for a wave-length λ and k' for λ' the amount of ozone is

$$x=\frac{k-k'-(\beta-\beta')-(\delta-\delta')}{a-a'} \quad \ldots\ldots\ldots (36)$$

If enough time is available δ and δ' can be determined by measurements outside the O_3 absorption. Otherwise the dust extinction may be regarded as independent of the wave-length, $\delta=\delta'$, which gives at least approximate values. Eq. (33) shows that for the determination of k we should know h. As long as this is unknown we have to let $y \approx z$ which probably does not cause too much of an error, since $h << R$. Actually, as we shall see later, this procedure can also be used to estimate the height of the O_3 layer.

By means of the foregoing method Fabry and Buisson[86, 85] have shown that the absorbing substance is really ozone. They measured the intensity of eleven different wave-lengths in the Hartley region of the solar spectrum. Then x, the equivalent amount of the absorbing substance, can be determined if the absorption coefficient a is known. Using the absorption coefficient for ozone, the same value for x was found for every pair of wave-lengths, indicating that the assumption of ozone as the absorbing agent was correct, because it seems very unlikely that there should exist an unknown substance which has just the same absorption coefficient in the Hartley region.

If the values for the extra-terrestrial intensities of the solar spectrum are once known the number of observations required

can obviously be reduced. Photographic methods can be used for the measurements without knowing the varying sensitivity of the photographic plate for different wave-lengths, because a factor characterizing the sensitivity of the particular photographic plate evidently drops out since the ratios and logarithmic differences rather than their absolute values enter in the formulae.

Another method for the determination of the total amount of ozone was developed by Götz[87]. It is based on the empirical result that in the region between 2800 and 3200A the absorption coefficient of O_3 can be expressed by

$$\log a = 17.58 - .00564\lambda \quad (\lambda \text{ in } A) \ldots\ldots\ldots\ldots (37)$$

Now, the shortest wave-length λ_0 which can be obtained with a certain instrument may have the intensity

$$I(\lambda_0) = \frac{I_0(\lambda_0)}{M}$$

where $I_0(\lambda_0)$ is the extra-terrestrial intensity of this wave-length and $1/M$ the fraction which can just be detected with the instrument used. Of course M varies for different instruments. On the other hand, as is seen from eq. (34),

$$10^{-a_0 x \sec y} = M$$

where a_0 is the absorption coefficient for the wave-length λ_0, and $y = z$ for the sake of simplicity. A combination of this formula with eq. (37) gives for the shortest wave-length

$$\lambda_0 = 177 \log x + 177 \log \sec z + \text{const} \ldots\ldots\ldots\ldots (38)$$

The constant depends only on the apparatus used, and after it has been determined, eq. (38) permits the approximate determination of the ozone content by a single observation.

Before the results of these two methods are discussed means of determining the vertical ozone distribution will be described. If there is reason to presume that ozone is mainly concentrated in one layer whose thickness is small compared to h, its height above the surface, h can be determined by choosing different values and seeing for which of these $\log I$ most nearly approximates a linear function of $\sec y$ (cf. eq. (33) and (34)). It is assumed of

course that during the time of such measurements the ozone content does not change appreciably. Rosseland[88] has pointed out that this method is not very reliable and breaks down if ozone is not restricted to a comparatively thin layer.

Better results are obtained by the method of zenith-light[89] where, as the name indicates, not the direct sunlight but the scattered light coming from the zenith is measured. The direct solar beam has an optical path through the ozone layers equal to their vertical extent multiplied by sec y. (If the layers are deep the integral over this product has to be taken for more accurate considerations). It is different with light which is scattered from the clear blue sky, for instance from a vertical direction, since light scattered from all heights will contribute. In fig. 22 the contribution of layer A to the total zenith light depends on, (1) the density of air at this height, and (2) the absorption and extinction of the light before and after scattering. If we move to the higher layer B the amount of light received at the instrument from B would be smaller than that received from A since the air density decreases. On the other hand a larger part of the oblique path of the direct beam is replaced by the vertical path after the scattering. This tends to let the light scattered from B become more intense than that from A. We observe, of course, at the surface of the earth the integrated effect of the light scattered from all points in the vertical. But, as just pointed out, there are two counteracting effects, one of which tends to increase the light from one particular layer as we go upward considering contributions from each layer separately, and another one which tends to decrease it; a similar thing happens as we go downward. Thus, there will be a region, of limited extent, in which most of the light received by the observer has been scattered. This region may be called the "effective height" of scattering. From the foregoing it is evident that the effective height, besides depending on the absorption, must be a function of the zenith angle of the sun. It is higher when the sun is low, and higher too for wave-lengths which are more readily absorbed. Götz, Meetham and Dobson, for instance, use the wave-length 3110A and 3290A[90] for their height determination of ozone. The effective height for the radiation 3110A is always at greater altitudes than for 3290A

since the absorption of the first wave-length is larger. As the sun goes down, the effective height of 3110A will occur above the whole or at least the greater part of the ozone. Evidently then, the intensity in the region 3110A as determined from zenith light observation will decrease much more slowly with decreasing solar altitude since the direct solar beam no longer passes through the ozone layers and therefore the absorption by ozone does not increase further with increasing obliqueness of the sun's rays. The radiation 3290A being less rapidly absorbed has a lower effective height than 3100A and thus, will still be in the lower

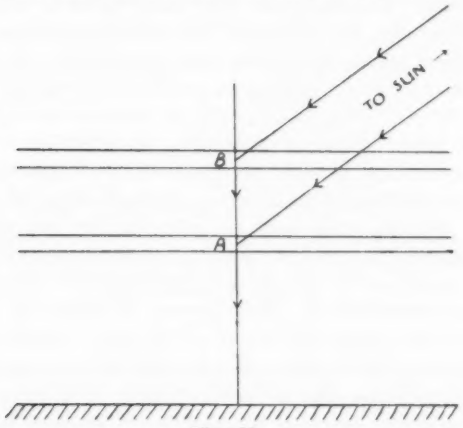


Fig. 22.

and richer ozone layers. Consequently, the intensity in the region 3290A still decreases fairly rapidly with the sinking of the sun. The result is that the intensity ratio I_{3110}/I_{3290} of the two wave-lengths first decreases with increasing zenith distance of the sun and reaches a minimum at a zenith angle of 85°, according to the observations, after which it increases. This phenomenon, first found by Götz[91], is generally called "Umkehr effect". It is possible to arrive at the vertical distribution of ozone by working backwards from the observed curves of the Umkehr effect[90]. This may be done by assuming different ozone distributions and computing the corresponding Umkehr curves. The Umkehr curve

which agrees best with the observations is evidently based on the hypothetical ozone distribution most closely resembling the occurence of ozone in the atmosphere. These calculations are quite laborious. The complicated mathematical theory has been studied by Chapman[92].

When the results of the observations are discussed it is necessary to keep in mind that the evaluation is based on the experimentally determined absorption coefficients. The experiments have been carried out under laboratory conditions which are very different from the conditions in the higher atmosphere. It is quite possible that under the conditions in these altitudes the absorbing power of ozone is different (variations with temperature and pressure) which of course would invalidate the numerical results.

The total amount of ozone in temperate latitudes is about 2.7 mm. on the average for the whole year; that is, if the total amount of ozone in the atmosphere could be concentrated at the surface it would make a layer of this height at N.T.P. The total number of ozone molecules above a square centimetre is therefore of the order of 10^{19}. There are considerable changes in the amount of ozone from day to day which are apparently correlated with weather changes. In particular the relation to pressure changes is well indicated[93, 94] as shown by the schematic fig. 23, according to Dobson. The ozone content is lower on the W. side of an anticyclone and higher on the W. side of a cyclone than on the average. The correlation coefficient between ozone and atmospheric pressure at 9 to 16 km. is $-.78$, the correlation with the temperature at 14 km. only .54. The correlation with the surface pressure is $-.46$. Similar results were obtained by Duckert[95]. It seems that with polar air the ozone content is higher, with equatorial air lower. It is still an open question as to whether the tropospheric air currents have an influence which reaches high enough to effect the ozone distribution by advection or whether the ozone content influences the weather. The first alternative seems on the whole more likely, especially for the day-to-day changes, as has been mentioned by V. Bjerknes[96].

A diurnal variation of the ozone content has not been found[97], but the yearly range is well pronounced, as fig. 24 shows for different

stations[94]. The maximum ozone content is found in spring, the minimum in fall, the amplitudes decrease with the latitude and the phase changes 180° at the equator. In India, where the amplitude is small, it seems that maximum and minimum occur at midsummer and midwinter.

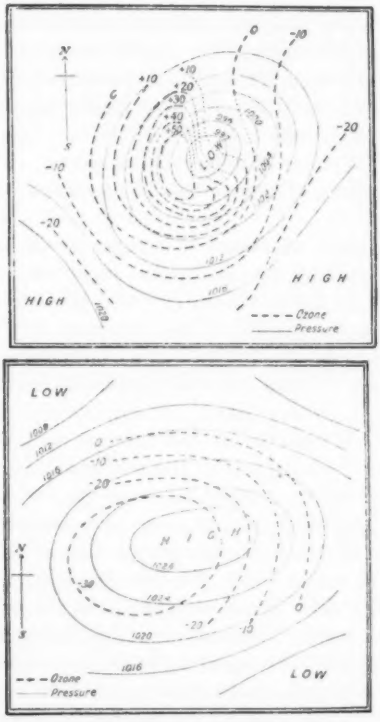


Fig. 23.—Schematic distribution of ozone in cyclones and anticyclones, after Dobson and others (Proc. Roy. Soc. Lond. A, vol. 122).

The geographical distribution of the ozone content which is indicated on fig. 24 is more clearly shown in fig. 25. The curves show high values in high latitudes, low values in tropical regions.

Our knowledge of the vertical distribution of ozone has undergone an important correction in recent years. It was concluded from earlier measurements that the main part of the ozone is in heights above 40 km. The more recent measurements by Götz, Dobson and Meetham[90, 98] with the zenith sky method and by

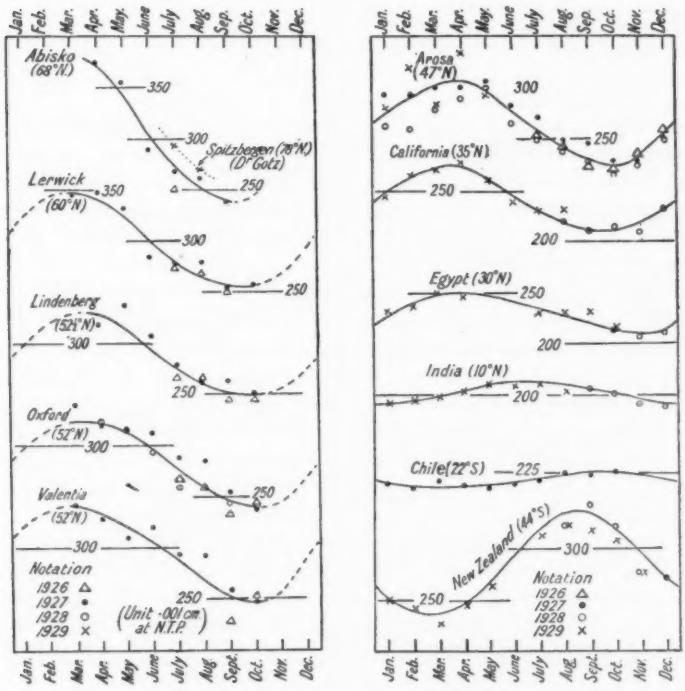


Fig. 24.—Yearly variation of ozone content in different latitudes, after Dobson (Proc. Roy. Soc. Lond. A, vol. 129).

Regener[99] with a more direct method, which will be described presently, have shown that by far the greatest part of the ozone is lower than 40 km. The ozone content of the atmosphere up to 3 km. altitude, which can be measured either directly by chemical

methods or by its light absorption, is very small, of the order of 1% of the total ozone content; and this value becomes relatively smaller if we consider that about 30% of the total mass of the atmosphere is below 3 km.

With the zenith light method Götz, Meetham and Dobson[90]

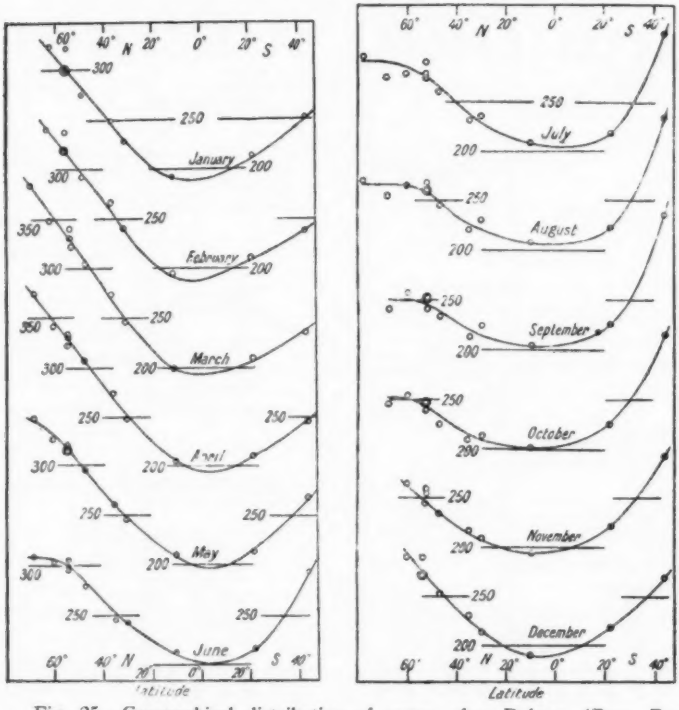


Fig. 25.—Geographical distribution of ozone, after Dobson (Proc. Roy. Soc. Lond. A, vol. 129).

have found the distribution of ozone above Arosa given in Table V, which is also characteristic for other places. The greatest absolute amount of ozone is contained between 25 and 30 km. Owing to the decrease of the atmospheric density with elevation the maximum of the volume percentage of ozone occurs at approximately 35 km.

TABLE V. Distribution of Ozone after Götz, Meetham and Dobson. Total amount .280 cm.

Height, km	2	5	10	15	20	25	30	35	40	45	50
$O_3 \frac{\text{cm.}}{\text{km.}} \times 10^{-3}$	5.8	5.9	6.4	7.3	8.7	9.5	9.1	6.46	1.73	.43	(.20)
$\frac{\text{vol. } O_3}{\text{vol. air}} \times 10^{-6}$	.07	.11	.21	.48	1.2	2.9	6.1	9.5	6.1	2.8	(2.3)

A similar distribution of ozone was found in Tromsö in Northern Norway, as shown by fig. 26[98]. The layer of maximum ozone content is lower at Tromsö than at Arosa, while the centre of gravity has the same height at both places. The ratio of O_3 volume to air volume has its maximum at a higher level.

It has been mentioned already that the new results regarding the distribution of ozone which were obtained by the zenith method are borne out by E. and V. H. Regener's measurements[99]. They sent unmanned balloons with automatic cameras up to heights above 30 km. and obtained photographic spectra of the sun which showed, by the displacement of the ultra-violet end of the solar spectrum, how much of the total ozone the balloon had passed at any height. Thus it was found that on July 31, 1934, at 30 km. 70% of the ozone was below the instrument.

Ozone as a powerful absorber of solar radiation determines to a large extent the temperature of the upper atmosphere. Gowan[100] has computed the vertical temperature distribution from the data regarding the ozone content of the different layers. Let $R_{\lambda T}$ be the radiation intensity of a sufficiently thin layer of air (assuming homogeneity in the horizontal direction) regarded as a black body for a certain wave-length λ and at a temperature T. Let $K_{\lambda z}$ and $K_{\lambda w}$ be the fractions of normally incident radiation which are absorbed by ozone and water vapour, respectively. Let S_λ and s_λ be the portion of direct and scattered sunlight, E_λ the portion of the earth's radiation, and X_λ and Y_λ the atmospheric radiations from above and below reaching the element. Since the element under consideration emits radiation in both directions, upward and downward, its emitting surface is, so to speak, twice as large

as its absorbing surface which accounts for the factor 2 in the following equation (39) which represents the equilibrium condition for emitted and absorbed radiation.

$$2\int_0^\infty R_{\lambda T}d\lambda = \int_0^\infty K_{\lambda s}(S_\lambda + s_\lambda + E_\lambda + X_\lambda + Y_\lambda)d\lambda + \int_0^\infty K_{\lambda W}(S_\lambda + s_\lambda + E_\lambda + X_\lambda + Y_\lambda)d\lambda \quad \ldots . (39)$$

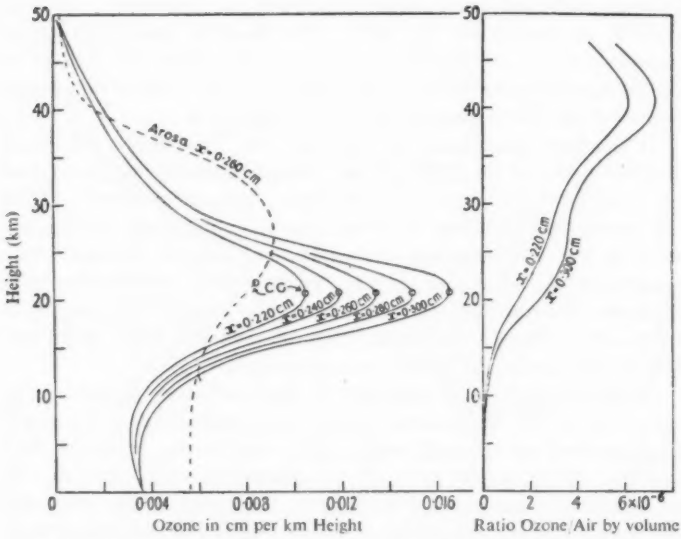


Fig. 26.—Vertical distribution of ozone, after Dobson and Meetham. x is the total ozone content assumed for each curve. *C. G.* is the centre of gravity. (Proc. Roy. Soc. Lond. A, vol. 148).

A correction has to be applied since eq. (39) is valid only for a parallel beam and normal incidence while we have here diffuse radiation. The computations are very laborious since the radiation intensity depends on the vapour content which in its turn depends on the temperature so that a method of trial and error has to be used.

Gowan's earlier calculations were based on the older results, according to which the increase in ozone content began above 40 km., and a marked rise in temperature at these altitudes was found. The work has been repeated recently (according to a kind verbal communication by Dr. Gowan) using the new information about the presence of ozone in lower stratospheric layers. The temperatures in the lowest layers for which computations were made agree well with the observed temperatures. Up to 35 km. the temperature rises only slightly above the values which are known by direct measurements (see fig. 1), then the temperature increases sharply to a maximum at 45 to 50 km. We shall see in the next chapter that the evidence of the skip distance of sound likewise suggests a rise of the temperature in these layers. The temperature would be the higher, the smaller the water vapour content assumed. The rate of cooling during the night was also estimated by Gowan and found to be quite appreciable. This would indicate that the night temperature in the high atmosphere might be rather low in the polar night.

The formation of ozone in the earth's atmosphere was originally assumed to be due to the action of ultra-violet light from the sun. When it was found that the ozone content is highest in spring and in polar latitudes it seemed that the action of the solar radiation consisted rather in the destruction than in the production of ozone, as for instance radiation in the Hartley band does. The maximum of ozone in high latitudes suggests that the generation of ozone by corpuscular radiation might be the explanation. But in this case ozone should be formed at the lower aurora level and its descent would require rather long time during which a great part of the ozone molecules probably would be destroyed again. Moreover, if this were the case a connection should exist between the amount of ozone and the magnetic perturbations and auroral displays, which Götz could not find during his Spitzbergen observations[91].

Chapman[101, 102] has shown on the other hand that it is possible to explain the annual variation of ozone as an interaction between the decomposing and generating effects of the ultra-violet solar radiation. Vertical or horizontal convection, which latter sometimes has been made responsible for the annual period and the

geographical distribution, were not taken into account. The band between 1300 and 1800A decomposes O_2, the Hartley band O_3. An estimate shows that the O atoms produced from O_3 are much more numerous than those produced from O_2. The rate of recombination of the type $O+O_2=O_3$ can be estimated from the absence of a diurnal variation. If all O atoms joined up with O_2 to form O_3 we would get a rise in the ozone content. Some, however, will combine with O or O_3 and form O_2. Chapman has shown that it is possible to attribute values to the different numerical quantities involved, in such a way that no daily but a yearly variation results. An essential feature of the theory is that the probability of reaction in a collision $O+O_3$ is less than in $O+O$, an assumption which could be investigated in the laboratory. The normal thermal decomposition $2O_3=3O_2$ is considered insignificant since the temperature in the ozone layers would not have the required value. It is an interesting consequence of Chapman's theory that atomic O will be present in the atmosphere in an increasing percentage as one goes upwards from the O_3 layers[103]. This must be so since the reactions $O_2+O=O_3$ and $2O=O_2$ are three-body collisions, the third particle being necessary to fulfil the condition of conservation of momentum and energy. Evidently the chance for such three-body collisions decreases upward with the density, so that the amount of atomic oxygen generated by the ultra-violet solar radiation will increase. Chapman concludes from a numerical discussion that the ozone concentration diminishes rapidly with the height above the level of maximum concentration and that the atomic oxygen increases until it exceeds O_2. The importance of this with regard to the composition and density of the upper atmosphere will be discussed in greater detail in the last chapter.

IX. Anomalous Propagation of Sound

For the study of the comparatively low levels in which ozone is observed we have another tool in the investigations into sound propagation. This method is very similar to the seismological study of the interior of the earth. In both cases we are dealing with the propagation of waves in an elastic medium. From the observable data conclusions are drawn about the elastic properties

of the medium. In the investigation of the seismic oscillations in the earth, however, longitudinal and transverse waves are studied, while in the "atmospheric seismic" we are only concerned with longitudinal waves, audible or inaudible. We have also transverse waves in the atmosphere, of great importance for meteorology, as for instance the wave perturbations which are the first stage of cyclonic development, but these do not concern us here.

There is an immediate practical interest in the propagation of sound in the lower troposphere. The audibility of fog signals is sometimes disturbed by peculiarities in the distribution of temperature and humidity. In times of war observations of the audibility of gun fire allow the location of the guns to be determined[104]. But this part of meteorological acoustics will not be dealt with here.

During accidental and artificial explosions it was observed that the zone of audibility around the source of the sound was not a simple more or less circular area but that there were one or more annular zones of audibility separated by zones of silence. Fig. 27 gives as an example the distribution of positive and negative reports of audibility (plus and minus signs) during an artificial explosion near Berlin in central Germany in winter.

To understand the origin of this distribution of zones of silence and audibility we have to consider the paths of the sound rays, *i.e.*, the normals to the wave-front, in the atmosphere.

The velocity of sound v in air is given by

$$v = \sqrt{\kappa \frac{R}{m} T} \quad \ldots\ldots\ldots\ldots\ldots\ldots (40)$$

where κ is the ratio of the specific heats for a diatomic gas, $R = 83.15 \times 10^6$ erg grad^{-1}, the gas constant; and $m = 28.9$, the molecular weight of air. For dry air the sound velocity at 273°K. is 331.3 m/sec. In the case of moist air the influence of atmospheric humidity has to be taken into account in eq. (40), but in greater elevations, the moisture content of the air is always very small. Since the velocity varies with temperature, and the temperature is mainly a function of the height, all rays except the one in the vertical will be curved. This curving of the ray is shown in fig. 28 where z represents the vertical and x horizontal direction. It

may further be assumed, that there are no temperature changes in the horizontal direction, so that the path of the sound lies in a vertical plane. This assumption, while of course not perfectly accurate, is quite justified because the temperature gradient in the vertical direction is almost always very much larger than in the horizontal direction. If the atmosphere is subdivided into sufficiently thin layers, 1, 2, . . . in each of which the temperature,

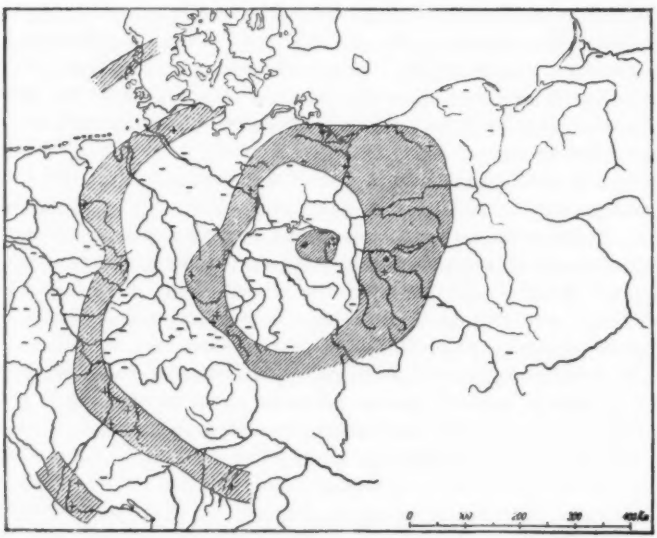


Fig. 27.—Zones of audibility at the explosion in Kummendorf, Germany, on December 18, 1925, after Duckert (Erg. kosm. Physik, vol. 1).

and therefore the sound velocity, is constant; and if ϵ_1, ϵ_2, . . . and so on are the angles of incidence and refraction, it follows from the law of refraction

$$\frac{v_1}{v_2} = \frac{\sin \epsilon_1}{\sin \epsilon_2}, \quad \frac{v_2}{v_3} = \frac{\sin \epsilon_2}{\sin \epsilon_3}, \ldots$$

From these expressions it is seen that if the temperature, and therefore the sound velocity, decreases, as is the case in the tro-

posphere, the sound ray is curved upward; if the temperature increases upward the sound will come back to the earth. This, incidentally, explains the good audibility in calm clear nights when the surface layers of the atmosphere are cooled by radiation and thus produce a strong inversion above them.

The preceding equations can be written

$$\frac{v_1}{\sin \epsilon_1} = \frac{v_2}{\sin \epsilon_2} = \ldots = \text{const.}$$

Total reflection occurs if

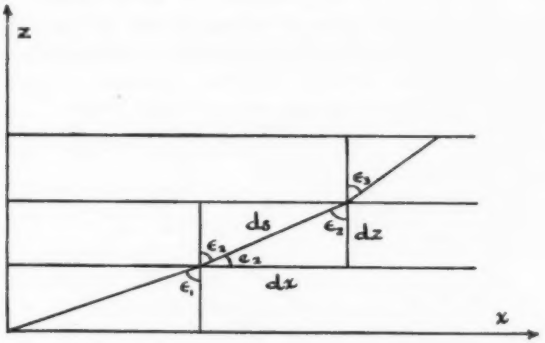


Fig. 28.

$$\sin \epsilon_1 \geqq \frac{v_1}{v_2} = \sqrt{1 - \frac{\Delta T}{T}},$$

so that at a temperature $T = 270°$K. total reflection takes place at the following angles of incidence and temperature inversions

ΔT	4	8	12	16° C.
ϵ	83.0°	80.2°	78.1	76.3

The incidence of the sound ray obviously has to be very close to the horizontal to give total reflection, even if the temperature inversion is large.

If the emergence angle e, complementary to ϵ, is chosen

$$\frac{v}{\cos e} = \text{const.}$$

If we have a ray which bends back to the earth, $e=0$ at the highest point, which gives the constant in the preceding equation equal to V, the velocity at the vertex.

$$\frac{v}{\cos e} = V \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (41)$$

The path of the ray can be found from the relations

$$\frac{dz}{ds} = \sin e; \quad \frac{dx}{ds} = \cos e \quad \ldots\ldots\ldots\ldots\ldots\ldots (42)$$

which are obvious from fig. 28. The time that it takes the sound to travel from the source to the place where it is heard or recorded, the travel time, is

$$\tau = 2 \int_0^{z_{max}} \frac{ds}{v(z)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (43)$$

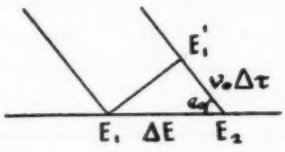


Fig. 29.

where z_{max} is the maximum height to which the sound reaches. Owing to our assumption that the temperature is a function only of the height, the path is of course symmetrical at the vertex. The distance E, at which the sound comes back to the ground, can be computed in a similar manner. The travel time can be observed and plotted as a function of E. In this fashion a travel time curve is obtained which plays a similar important rôle in seismology and atmospheric acoustics.

Consider two rays returning to the earth's surface at the points E_1 and E_2. (Fig. 29). When the wave-front touches the surface at E_1 it is also at E'_1, and the time $v_0 \Delta\tau$ will be required before the sound is heard at E_2. It is

$$\Delta E = \frac{v_0 \Delta \tau}{\cos e_0} \quad \ldots\ldots\ldots\ldots\ldots\ldots (44)$$

or, with the aid of eq. (41),

$$\frac{\Delta E}{\Delta \tau} = V \quad \ldots\ldots\ldots\ldots\ldots\ldots (45)$$

Thus if the observations allow the construction of a travel time curve it will be possible to determine the velocity at the highest point of the path of the sound. The height of this vertex of the sound ray can also be determined. The observed distribution of temperature and wind permits the construction of the sound ray up to the highest point of meteorological information. For the higher part of the sound path somewhat arbitrary assumptions have to be made, but the choice is limited since the actual time and the horizontal distance travelled in the unknown region are known[105]. Theoretically it is even possible to compute the height

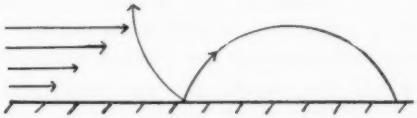


Fig. 30.—Influence of vertical wind distribution on the propagation of sound (schematic).

by the Wiechert-Herglotz method[106], but in practice the other method seems to give equally satisfactory results.

Superimposed on the influence due to temperature is the effect of the wind on the propagation of the sound. If the wind is taken into account the normal to the wave-front no longer coincides with the ray. For instance, if a plane sound wave-surface is propagated perpendicularly upward while there is a horizontal wind, the sound ray will be inclined. The ray is, of course, more important, since the transport of energy takes place along the ray. The influence of the wind itself is rather small; the important factor is the influence of the change of wind with the altitude. All these questions have been studied in great detail by Galbrun[107]. We shall just give a schematic picture here (fig. 30). If the wind

increases with elevation the oblique sound ray comes into regions with higher and higher total velocity (wind plus sound) so that the ray propagated in the direction of the wind is refracted more and more away from the normal and therefore is curved back to the surface if the change in the velocity of the wind is sufficiently great. On the other hand the sound ray in the direction opposite to the wind is becoming more and more steeply inclined against the surface of the earth, so that the sound will not be refracted back to the earth. These considerations which are obviously of great practical importance, for the audibility of fog signals for instance, show that that wind action, which by necessity must be asymmetrical, cannot explain the annular zones of anomalous audibility as shown in fig. 27. It might, however, explain asymmetries in the development of these zones.

It has been assumed so far that in a horizontal direction temperature and wind are uniform, so that the sound ray would lie in a plane. In some cases, however, especially near atmospheric fronts with their horizontal temperature contrasts, and owing to changes in the wind direction with elevation, the sound ray sometimes becomes a space curve, making the problem more complicated.

The first observations concerning the anomalous propagation of sound were made without instruments and consisted of reports as to whether or not a sound was heard, when the sound was heard and, possibly, from which azimuth and altitude, approximately, the sound seemed to come down to the observer.

The instruments used in Germany are all based on the principle of the movement of a piston membrane[108]. A little disk is set in a plate of large dimensions. This disk is fitted so that it can move perpendicularly to the large plate under the influence of incoming pressure-waves. A spring brings the disk back in its equilibrium position. The motion of the disk is recorded on photographic paper. In England the hot-wire microphone is applied to record the arrival of the sound[105]. Here the electric resistance of a hot wire is measured. The wire is placed in the orifice of a resonator. When the air in the resonator vibrates, some of it passes through the orifice, cooling the wire and lowering its resistance. This instrument allows the recording of sound waves at different stations

a few hundred metres apart on one record, thus giving more accurately the time-difference between the arrival of the sound waves at nearby stations. But it seems on the other hand more susceptible to the wind oscillations than the German instrument.

The first observations of skip distances and zones of anomalous audibility were occasional and made during accidental explosions. During the great war gunfire was heard better to the west in summer, and to the east in winter. From which it may be concluded that in winter the zone of audibility is better developed towards the east, in summer towards the west. There are exceptions to this rule[105] and it is not known if it also holds in southern Europe or in other parts of the world.

The varying direction of audibility during summer and winter in central and western Europe is obviously an effect of the wind distribution. Whipple[105] showed by means of the pressure distribution in higher altitudes of the stratosphere that the pressure differences between England and Lapland are such that there results east wind in summer and west wind in winter, just the directions which are required for the explanation of the zones of audibility. The transitions occur at the end of March and in the middle of September, which also agrees well with the sound observations.

Following these occasional observations systematic studies were begun after the war when great quantities of explosives had to be destroyed. In Germany a great net work of more than 500 voluntary observers was established. These observers were instructed about the approximate time when the sound was to be expected, and were asked to report the exact time of arrival of the sound, if any, and if possible the direction from which the sound seemed to come. During the first explosions a number of difficulties were experienced. One of the troubles was that a number of observers had the ambition to give a positive report of audibility under any circumstances. Such unreliable observers had to be sifted out by giving false information about the explosions and finding those stations which would report sound even if no explosions had taken place.

As a result of such observations the distribution of the zones of silence and audibility around a source of explosion was definitely established (fig. 27). Similar observations were made in England and other countries. The zones of audibility are not always as clearly separated from each other as in the figure. Sometimes overlapping takes place, evidently due to wind influence.

Entirely from ear observations the travel time curve can be obtained which, as we have seen, allows the computation of the velocity at the highest point. It is, however, often difficult to get accurate time differences between two stations at which ear observations are taken. A somewhat different way has been employed by placing three instruments at the corners of an approximately equilateral triangle with sides about 350 m. in length. The records of all three instruments are taken on the same paper so that the difference in time of arrival of the explosion wave can be obtained very accurately. The azimuth of the sound ray can be found since three stations are available. With the aid of eq. (44) and (45) the inclination of the sound ray and the velocity at the highest point can be obtained in the same way as from the travel time curve.

At the inner boundary of the zone of audibility the angle of descent is steep, and the sharp limit of the zone of audibility is to be understood as the result of a focal line there.

The time taken by sound to reach the zone of abnormal audibility is longer than it would require if only horizontal propagation with velocities corresponding to the surface temperatures took place. The delay ranges between 90 and 100 sec. and is about the same for stations at different distances from the place of the explosion. This is, of course, due to the curved path along which the sound travels. In England, the maximum height of the path is about 40 km. in summer according to Whipple[105]. Sometimes it seems to be as high as 60 km., but in these cases the computation of the path is less reliable, since there is a larger part of the curve extrapolated owing to the lack of direct information. Thus the distribution of the velocity of sound can be obtained at levels higher than the balloons with instruments reach. Since the

observations show that the sound rays are bent back to the earth again the velocity at the vertex of the path must be larger than at the surface of the earth.

The possible influence of the wind on the propagation of sound can be eliminated since the wind effect, as we have seen, is decidedly asymmetrical.

The high velocity of sound in the upper strata is at present generally interpreted as an indication of temperatures higher than those at the earth's surface. Sometimes objections have been raised as to the validity of eq. (40) throughout the whole path. This equation holds when the pressure amplitude is small compared to the mean value of the pressure. If the amplitude becomes comparatively large the velocity increases, according to Riemann. Wiechert[109] has shown that these objections are untenable. Eq. (40) shows that the high velocity may be caused either by high temperatures or by an increase in the ratio of the specific heats or a decrease in the molecular weight of the air. The third possibility would mean the beginning of a preponderance of light gases (H, He) at heights of about 40 km., which is difficult to reconcile with the results previously mentioned that nitrogen and oxygen are present at great heights while no indications of the lighter gases have been found. But even if lighter gases should be present at higher altitudes, it seems very difficult to visualize how they could be present in a sufficiently large percentage at 40 km., in view of our present knowledge that turbulence and mixing extend well up into the stratosphere. According to Wiechert[109] the atmosphere must consist of more than 25% of hydrogen at 40 km. to explain the return of sound waves by the change of the molecular weight of the air.

The other explanation[105], change in the ratio of the specific heats, has an interest especially in view of Chapman's theoretical result[103] that above the level of maximum ozone content atomic oxygen should become more and more important. Since the ratio of the specific heats of monatomic gases is larger than of diatomic gases, we might have here an alternative explanation for the increase in the velocity of the sound.

However, at present the explanation by means of high tempera-

tures seems to be the most widely accepted one, especially since we know through Gowan's work[100] that the postulated high temperature can be explained by ozone. The following Table VI gives

TABLE VI. Summer temperatures (°K) from sound observations

Height (km.)	20	25	30	35	40	45	50
Whipple	219°	220	223	245	282	310	336°
Duckert	219°	220	223	267	316	335	344°
Gutenberg	219°	220	223	256	306	330	349°

summer temperatures derived from sound observations[106] by different workers in England and Germany. While the actual values of the temperature are somewhat different from each other, owing to the necessarily hypothetical assumptions in the higher layers, all values show a marked increase above 35 km. and are considerably higher than the surface temperatures.

Our information about sound propagation in other parts of the world is naturally very incomplete, but anomalous propagation of sound has been observed in the tropics and in the arctic circle in summer and winter. Wölcken[110] recently reported the results of the observations of 12 explosions at Russian arctic stations during the polar night. The preliminary study of the results showed anomalous propagation of sound at three explosions. If it would become definitely established that even during the polar night the bending back of the sound rays takes place it would be difficult to explain the phenomenon by high temperatures. To account for the heating of the atmosphere by corpuscular radiation during the polar night seems hardly satisfactory. It may be more likely that warmer air from lower altitudes is continually transported polewards, so that the air does not stay there long enough to cool appreciably. Perhaps it will be necessary to resort to the explanation of the higher sound velocities by means of an increasing ratio of specific heats as suggested by Whipple[105].

(To be concluded)

METEOR NEWS

Observations relating to meteors and meteorites are cordially invited.

The Observations of the Perseid Meteors in 1936

(With plate III).

Stations in Eastern Canada were fortunate in having generally clear weather on two of the best nights for Perseid observation in 1936, Aug. 10-11 and Aug. 11-12. As in other years a group observed at the Dunlap Observatory and a number of reports were received from other groups most of whom had previously assisted with summer meteor observation. The three stations established by the Ottawa Observatory were particularly successful in both the visual and photographic work and the results of these programmes, as reported by Miss Burland and Mr. Thomson, who were in complete charge of the Ottawa stations, will be found elsewhere in these notes. Other centres of the Society which continue to show enthusiasm in the collection of meteoric data are the London group, under Mr. Middlebrook, the Hamilton observers, headed by Prof. Findlay, and a group of scouts under Mr. Paterson from the Montreal centre. It is the writer's hope that eventually all centres of the Society will contribute actively to this branch of astronomy which is most suited to amateur observation. If anyone wishes to form a group for summer observation in 1937 full information and instructions may be obtained by writing to this department of the Journal. In addition to the parties mentioned above, observations were again received from Dwight and from several new stations.

The personnel of the various observing groups is listed in Table I. The observations were carried out in much the same way as for previous programmes, the only real difference being that, in this case, the observers were not requested to plot the meteors fainter than zero magnitude but were asked to emphasize magnitude determination and train observation. Actual numbers of meteors observed will be found in Table II. Here, in some cases, considerable uncertainty arises in the division of the meteors into Perseids and Non-Perseids, which may explain some apparent inconsistencies in the observed rates. The hourly rates for a

TABLE I

PERSEID OBSERVATION GROUPS, 1936

1. *Dunlap Observatory*—A. H. Armstrong, Miss M. Chambers, Miss M. Eddy, Miss V. E. Foster, R. J. James, D. A. MacRae, Miss M. McKinley, D. McKinley, P. M. Millman, Miss M. J. Thompson.
2. *London, Ont.*—Major E. H. Anundson, Rev. W. A. Colgrove, Mrs. A. Davies, Miss Marion Ellis, Prof. H. R. Kingston, J. M. Kingston, Dr. Gordon Magee, J. Middlebrook, Miss Emma Rowley, G. Rowley, Miss B. Taylor.
3. *Camp Bois Franc, Que.*—D. Bright, A. Brown, F. Corkran, G. Corkran, W. Martin, I. Paterson, E. Russell Paterson.
4. *Dwight, Ont.*—Mrs. R. M. Millman, Mrs. J. H. Millman, J. H. Millman, R. N. Millman.
5. *Hamilton, Ont.*—Prof. Wm. Findlay, T. H. Wingham.
6. *Windsor, Ont.*—Eugene Duchesne.
7. *Camperdown, Ont.*—R. B. Laing, M. I. Laing.
8. *Acton, Ont.*—Neil McNabb, Jr.
9. *Barrie Beach*—Miss E. Delane, Miss E. Jaques, Miss R. J. Northcott.

number of the groups, reduced to six observers, will be found plotted in Figure 1. A marked increase in the Perseid rate for the second night is clearly indicated. Though the rates plotted in the figure are comparable with those found in 1935 it must be remembered that in 1935 there was a moon very near the full while in 1936 the moon was well past the last quarter. Because of this fact it would seem that the Perseids in 1936 were considerably less numerous than in either 1935 or 1934. The magnitude distributions are given in Table III. There was a marked absence of very bright and spectacular meteors.

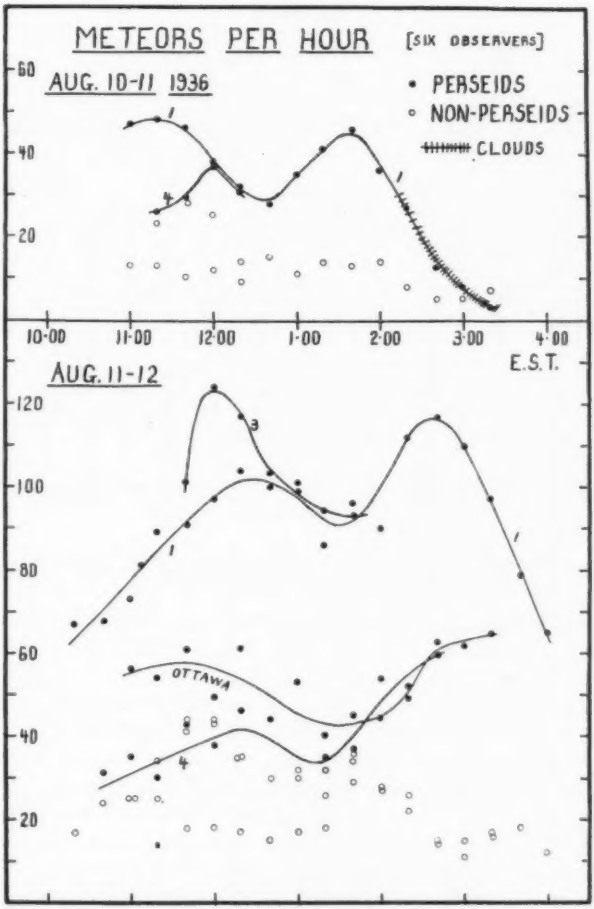


Fig. 1.—Hourly rates of the Perseid meteors in 1936. Each plotted point represents the reduced rate taken over a complete hour centered at the time indicated. The numbers accompanying the curves refer to the groups listed in Table I. The drop in the rates for most groups during the early morning hours may be in part owing to the rising moon which appeared near midnight on the two nights in question.

TABLE II

Date	Aug. 10-11		Aug. 11-12											
Group	1.		1.		2.		3.		4.		Ottawa		Hull	
E.S.T.	P	NP	P	NP	P	NP	P	NP	P	NP	P	NP	P	NP
9.30- 9.50					1	1								
9.50-10.10			4	0	2	2								
10.10-10.30			12	4	3	4			8	3				
10.30-10.50	8	1	11	6	5	5			8	5	16	7	1	5
10.50-11.10	12	6	24	6	6	4			3	2	16	10	8	15
11.10-11.30	13	3	26	8	6	4	17	9	10	3	24	8	13	23
11.30-11.50	16	2	29	17	8	3	42	19	5	3	14	7	9	14
11.50-12.10	12	4	28	13	8	5	42	16	12	4	23	3	13	16
12.10-12.30	9	6	31	10	11	2	40	9	10	4	12	8	9	22
12.30-12.50	9	4	36	9			35	10			21	5	6	9
12.50- 1.10	7	5	27	7			28	11			4	1	2	9
1.10- 1.30	13	2	32	14	5	6	36	9	5	1	11	5		
1.30- 1.50	13	6	22	9	10	7	30	6	7	5	9	5		
1.50- 2.10	13	4	34	10	13	5	27	14	10	2	9	14		
2.10- 2.30	4	2	24	5	3	4			11	0	20	4	29	20
2.30- 2.50	5*	1	17	2					3	0	18	5	30	14
2.50- 3.10	0*	1	34	3					8	5	25	5	23	18
3.10- 3.30	1*	2	18	5							19	1	9	9
3.30- 3.50	1*	2	30	6							21	10	7	1
3.50- 4.10			20	4									3	0
4.10- 4.30			4	0										
Totals	136	51	463	138	81	52	297	103	100	37	262	98	162	175
Average no. of observers	4		5		4		6		2		5		6	

Date	Group	Time Observed E.S.T.	P	NP	Observers	Average Hourly Rate (6 obs.)	
Aug. 7- 8	9	9.00-10.00	3	3	1	10	10
8- 9	9	9.00-11.10	4	9	1	7	15
Aug. 10-11	2	9.50-11.30	9	5	1	19	11
10-11	4	11.05-12.45	32	22	3	28	19
10-11	6	10.30- 2.20	23	3	1	21	3
10-11	8	10.00- 1.15	0*	1	1		
10-11	Ottawa	10.45-12.45	7*	10	6		
10-11	Hull	10.25-11.45	17*	7	6		
10-11	Golden Lake	10.15- 1.50	14	8	1	14	8
Aug. 11-12	5	12.00- 2.00	50	20	1	80	35
11-12	6	10.30- 4.45	80	6	1	45	3
11-12	7	12.50- 3.10	69	6	1	104	9
11-12	8	8.20-11.40	4	2	1		
11-12	Golden Lake	10.15-12.30	30	4	1	47	6
Aug. 13-14	1	10.50-12.30	12*	2	2		

Total perseids observed (P) 1855.
Total non-perseids observed (NP) 762.
Total meteors observed 2617.
*Clouds.

TABLE III

MAGNITUDE DISTRIBUTIONS, AUGUST 10-12, 1936

Mag.	−4	−3	−2	−1	0	1	2	3	4	5	Totals	Mean Mag.
Perseids												
Ottawa and Hull			4	4	32	90	123	97	61	5	416	2.12
D.D.O.	1	7	14	36	85	181	153	90	17	1	585	1.26
Groups 2, 3, 4, 6, 7		1	13	30	63	124	149	138	61	12	591	1.83
Non-Perseids												
Ottawa and Hull		1	2	8	8	43	77	91	52	8	290	2.43
D.D.O.			4	5	15	47	55	33	21		180	1.82
Groups 2, 3, 4, 6, 7			2	6	13	46	43	55	27	4	196	2.11

METEOR OBSERVATIONS MADE BY THE OTTAWA CENTRE IN AUGUST, 1936

By Miriam S. Burland and M. M. Thomson of the Dominion Observatory

The Ottawa centre was responsible for meteor observations from three locations: (1) The Dominion Observatory with Miss M. S. Burland in charge; (2) The Department of Agriculture Research Station, Hull, Quebec, with Mr. M. M. Thomson in charge; (3) Golden Lake, with Mr. F. W. Matley in charge. The first two locations were separated by a base-line of 3.7 miles, and were chosen so that the whole sky could be observed without the need of either group looking over the city lights. This arrangement seemed better than that of previous occasions when the whole sky was observed from one location.

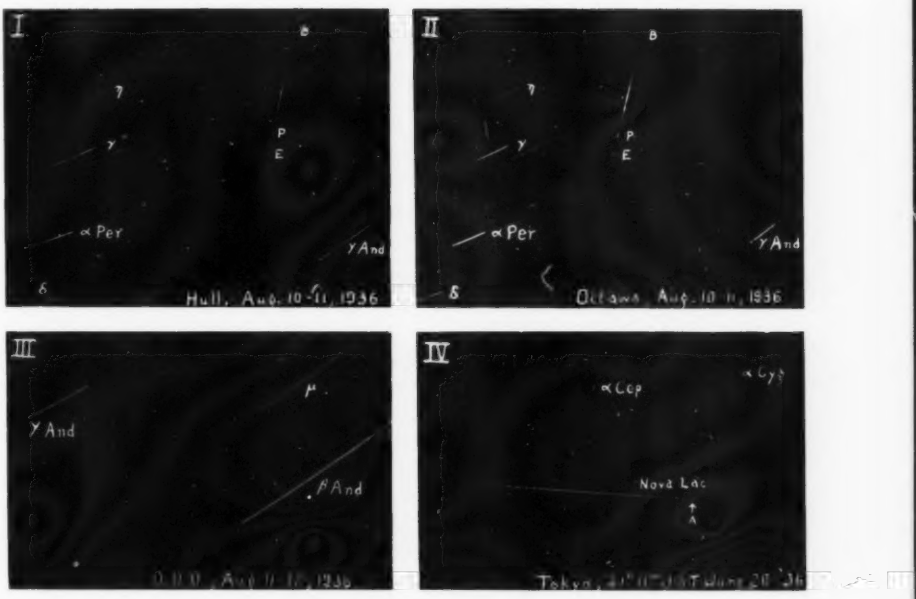
In place of stop watches which were used formerly, the Ottawa station (group (1)) used a seconds dial operated electrically from the Observatory time circuits. At Hull (group (2)) a chronometer, rated to within a second day and checked by radio with the Observatory time signals, was used. At Golden Lake Mr. Matley used his pocket watch which was correct to within a few seconds. In addition to the visual observations, a camera was focussed on the radiant from each location. The necessary film rolls and packs

were supplied by the Observatory through the kindness of Mr. R. Meldrum Stewart, Director.

The assistants at the Ottawa-Hull stations were: Ladies—Marion Mitchell, Audrey Mitchell, Dora Jolly, Gladys Jolly, Margaret Russell, Lora Cairncross, Margaret Lafleur, Eileen Tuck, Lena Ellichuck, M. S. Burland; Gentlemen: Robert Sims, Frank Legge, Maurice Carriere, Lucien Carriere, Sam Palef, John Sydon, Laird Nicholson, Frank Beasley, Leo Leblanc, Robert Villeneuve, Karl Buckthought, W. R. Knowles, W. D. McLennan, M. M. Thomson. These are for the most part students of Mr. W. J. C. Hyland of the High School of Commerce, Ottawa. Thanks are due to him for arousing an interest in so many young people. Some had previous experience, but for the most part they were new observers. There was a sufficient number to form two groups of seven for each of five nights, six observers and one recorder. In addition Mr. W. D. McLennan operated his camera each night at the Observatory, while Mr. M. M. Thomson did the same in Hull. At Golden Lake, Mr. Matley was assisted by Mr. G. H. Gale. But, as he suggested afterwards, a party of two is scarcely sufficient for good results.

From the Ottawa and Hull stations, only two of the five nights scheduled were suitable for observing. The one, Monday, August 10-11, finally clouded over and started to rain; the other, Tuesday, August 11-12 was clear throughout, but accompanied by a heavy penetrating dew. This accounts for the break in the Hull records. during which time the group went in to have lunch and to warm up, The Ottawa group, too, was reduced at times from six to two observers. Thus the observations actually obtained, 360 at Ottawa and 337 at Hull, a total of 697, represents by no means a perfect count. Experienced observers could easily have recorded 1,000 meteors from the two locations. Details of the counts will be found in Table II.

Of the two nights of observing at Golden Lake, Mr. Matley states that the first was quite clear, cold with a heavy dew, and quite devoid of Perseids. The second night was equally clear, warmer, and much more satisfactory from the standpoint of Perseids but due to flashlight failure part way through, he was forced to retire. The third night proved cloudy.



FOUR INTERESTING METEOR PHOTOGRAPHS TAKEN DURING THE SUMMER OF 1936

I and *II* are the photographs the meteor whose height has been determined by Miss Burland and Mr. Thomson (see text). The relative displacement of the meteor as observed from the two stations is easily seen. *III* is the segmented trail of a Perseid photographed at the Dunlap Observatory. Six segments are visible on the original negative and, since the segments are 0.04 seconds apart, the complete photographic trail was covered in 0.2 seconds. *IV* is a rather unique meteor photograph sent in by Dr. M. Huruhata of the Tokyo Observatory. It was taken by Japanese middle school students on the night of the maximum of Nova Lacertae, the diurnal trail of this star crossing the meteor path as indicated in the reproduction. Though the sky was quite cloudy at the time the meteor appeared, it was estimated as of apparent magnitude −6, motion slow, the 60 degrees of visual path being covered in 6 seconds. The colour at first was green but two-thirds of the way along the visual path it split into two pieces, one blue-white in colour the other red. The train was reported as very remarkable. The meteor was moving south, that is, from left to right as reproduced. The first part of the photographic trail shows a series of semi-periodic bursts, indicating rotation. The latter part of the trail is quite faint, owing chiefly to clouds. On the contact print the division into two parts can easily be distinguished at "A". There is also a faint luminescence (not reproduced) on either side of the trail for some distance back of "A". This may possibly be the persistent train but is more likely reflection of the meteor's light from clouds.

The Ottawa and Hull records show some agreement in the early part of the night of August 11-12, but only in the Hull records is there a marked increase in Perseids around 2.00 a.m. This may be due to Hull observers facing the north and east while the Ottawa observers faced south and west, away from the radiant. A combination of the Ottawa-Hull results still shows the maximum. A greater proportion of non-Perseids were recorded in Hull during the first half.

A comparison of magnitudes shows the Ottawa-Hull stations to be in close agreement, particularly in view of the number of new observers. At Golden Lake, Mr. Matley was concerned more particularly with charting only the brightest meteors, and had at the same time to look after his camera. Hence the mean magnitude from his observations is scarcely comparable to that of Ottawa-Hull mean magnitudes. Magnitude distributions are given in Table III.

Photographically, Mr. Matley was unsuccessful, owing perhaps in part to the heavy dew which he suggests might have fogged his lens. At Ottawa two trails were recorded, while in Hull the duplicate to one of the Ottawa trails was also recorded, thus providing excellent material for obtaining not only the height and distance, but also the radiant point. The meteor which was photographed at both stations appeared at $23^h\ 9^m\ 22^s$ E.S.T. Aug. 10, 1936, and was estimated as of magnitude -1 and -2 respectively at the two stations. It was a non-Perseid. The two cameras with which the photographs were taken were almost identical, each being adapted for film pack, $2\frac{1}{4}''$ x $3\frac{1}{4}''$, and equipped with an F/4.5 lens. The one at the Ottawa station was in charge of Mr. W. D. McLennan while that at Hull was operated by Mr. M. M. Thomson.

Both films were later mounted between glass and measurements made. In order to obtain as accurate readings as possible, the following star positions were measured, η Persei, θ Persei, 51 Andromedae, and γ Andromedae, also the two bursts or explosions near the end of the trail, lettered *P* and *E*.

The films were placed in such a way that the star trails at the plate centre were parallel to the motion of the micrometer screw, giving right ascension readings, while the declinations were measured by turning the carriage through ninety degrees.

A least-square computation was carried out to solve for the plate constants in each case. With these constants it was possible to compute the right ascension and declination for E and P as seen from both stations, and these in turn, with the local sidereal time for each station, gave the altitude and azimuth for E and P as observed from Hull and from Ottawa.

With this information the heights of the disappearance point of the meteor (E) and the point on the trail (P) were obtained. The mean values are:

Height of E (end of meteor) 89.9 km.
Height of P (point on trail) 95.6 km.

The beginning of the photographic trail was at a height of approximately 120 km. The distances from E to Ottawa and Hull were computed to be 161.2 km. and 159.8 km., while P proved to be 166.9 km. and 165.6 km. away.

Owing to the fact that the explosion point P was so definitely marked on both trails, it was possible to determine the radiant point of the meteor. The mean position for this was computed as $23^h\ 16^m$ R.A., $+62°\ 10'$ Decl., a point very close to the boundary between Cassiopeia and Cepheus and which may be located by extending a line from α Cassiopeiae through β Cassiopeiae to a distance slightly greater than that between α and β.

Computations were also made to determine the point on the earth toward which the meteor was speeding when it burned out. The results gave this point as slightly east of north east from both stations and at a distance of approximately 85 km.

All computations involved were equally divided between Miss M. S. Burland and Mr. M. M. Thomson, the measures being made by the former. In connection with the work thanks are due Mr. R. Meldrum Stewart, director of the Dominion Observatory, for his advice and whole-hearted co-operation.

The Photographic Observations

In addition to the photographic programme carried out at the Ottawa Observatory and described above by Miss Burland and Mr. Thomson, cameras were exposed at several other stations during

the time of the Perseid maximum. At the Dunlap Observatory three spectrographs of speed F/4.5 and one direct camera, F/2.0, were operated by the writer. Two of the spectrographs, covering adjacent fields, and the direct camera were mounted behind a rotating sector 17 inches in diameter. This was driven by a synchronous motor and gave 25 occultations per second, the time of a single exposure being 1/75 second. Ilford Astra IX plates were used in the spectrographs and Ilford Hypersensitive Pan roll film in the direct camera. The spectrographic programme was not productive of results, owing chiefly to the lack of any very spectacular meteors, but a segmented meteor trail showing six segments was photographed on Aug. 11-12 with the F/2.0 lens. The photograph is reproduced in Plate III. Unfortunately the meteor cannot be identified in the visual records made at the Dunlap Observatory. Mr. D. McKinley also exposed a lens of speed F/6.3 at the Dunlap Observatory but secured no meteor photographs.

Mr. Jack Grant of Orillia, Ont., made an excellent series of photographic observations in collaboration with the Dunlap Observatory. He used a 16 mm. movie camera equipped with a lens of speed F/1.9. It was loaded with Eastman Super X Pan negative film and each frame was exposed from 5 to 22 minutes depending on the sky luminosity. Five Perseid photographs were secured with this camera and a sixth with a lens of speed F/6.3. Mr. Grant is to be congratulated upon the efficient way in which his programme was carried out and on the results obtained. Though at first glance it appeared that Mr. Grant had photographed the same meteor as was secured at the Dunlap Observatory, more careful reduction showed that the two photographs could not be of the same object. Cameras were exposed without success at the Acton and London stations but the writer has no details of these observations.

Table IV contains a summary of the photographic results. An interesting account of a photographic programme carried out by Mr. Franklin W. Smith of Glenolden, Pa., during July and August of 1936 has recently been received. He used a Vollenda Kodak with lens speed F/3.5 which proved to be very efficient in meteor photography. His results are also included in Table IV. He gives the magnitudes of the two meteors he photographed as 0 and −1.

TABLE IV

PHOTOGRAPHIC OBSERVATIONS OF THE PERSEID METEORS

Station	Date	Total Exposure Time (hrs.)	Meteors Photographed	Camera Speeds		
Spectrophotography						
Dunlap Obs.	Aug. 10-16	44.4	—	F/4.5	F/4.5	F/4.5
Direct Photography						
Dunlap Obs.	Aug. 11-12	12.1	1	F/2.0	F/6.3	
	13-16	5.0	—	F/2.0		
Orillia, Ont.	Aug. 10-11	7.0	1	F 1.9	F/6.3	
	" 11-12	9.9	4	F 1.9	F/6.3	
	" 12-13	7.6	1	F 1.9	F/6.3	
Ottawa, Ont.	Aug. 10-12	5.2	2	F/4.5		
Hull, Que.	Aug. 10-12	4.8	1	F/4.5		
Golden Lake, Ont.	Aug. 10-12	5.3	—	F/4.5		
Glenolden, Pa.	July 15-Aug. 17	12.3	2	F/3.5		

Though the total Perseid observations made in 1936 do not equal in numbers those made in other years the writer feels that the groups this year were better organized and that the programmes were particularly successful, especially in view of the increased activity in meteor photography. It is hoped that this interest in photography will be still further augmented in the future. Sincere thanks is here expressed to all those who took an active part in the study of the Perseid meteors.

P. M. M.

REVIEW OF PUBLICATIONS

Interpolation and Allied Tables. Reprinted from the Nautical Almanac for 1937. 44 pp. His Majesty's Stationery Office. 1 shilling net.

The British Nautical Almanac for 1937 contains a very useful and practical set of tables to facilitate interpolation based on Bessel's and Everett's formulae; also for Lagrange's formula for interpolation without differences. Particular instruction is given on the use of the formula and the application of multiplying machines to facilitate the computations.

These tables have recently been reprinted as a separate pamphlet bound in a fairly heavy and durable paper cover. So reprinted they form a very convenient set of tables for anyone who has occasion to use interpolation tables.

R. K. Y.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

"Beyond the Stratosphere"—Reply to Criticisms

In the May-June 1936 Journal was an article by William Gifford Hale describing an imaginary flight to the moon, and in the following issue were two criticisms of it in which certain scientific inaccuracies were pointed out. Mr. Hale on account of illness was unable to reply earlier. Referring to the two matters to which exception was taken he says:

1. *Encountering meteorites in space.* Very possibly I have unintentionally exaggerated this danger. But remember meteorites do sometimes come in groups, even following one another in the same "line of fire". And one good puncture would let the air out through the car wall quite effectively enough.

2. *The turning over as the car approached the Moon.* I certainly did not think of air resistance, for well I know that air would be absent all the way across from earth to moon. I do not set up as an expert mathematician, so it

may well be that I have wrongly supposed that the projectile would turn its heaviest end downwards toward the moon as it passed the neutral point, at which the attraction of the earth and moon would balance each other. I think, however, that it is all a matter of the speed, and hence the momentum of the projectile when it attained that neutral point. If its speed was still great it might still hold on its way "point first", but if it was given at the start only enough force to reach that neutral point its motion thence forward would be only a fall, as from standstill, as when a stone is thrown up into the air: when its upward force is spent and it pauses and begins to fall, it will most certainly come down heavy end first, and I think the same would apply to our moon projectile. In that case it would have a 20,000-mile fall to the moon, and would certainly need reserve rockets, placed just as those which started it off to check that tremendous fall.

As everyone knows, H. G. Wells has published a number of fantastic scientific romances, and in 1933 eight of these were published in a single volume of which a second printing was made in 1935. To this volume Mr. Wells contributes a characteristic preface, wherein he describes the circumstances under which his stories were published, and also he contrasts them with those of Jules Verne, fifty years earlier (1865-70). Verne wrote and believed and told that this or that thing could be done which was not at that time done. Many of his inventions have "come true". On the other hand Wells's stories do not pretend to deal with possible things; they are exercises of the imagination in a quite different field. He says: "They are all fantasies; they do not aim to project a serious possibility; they aim indeed only at the same amount of conviction as one gets in a good gripping dream. They have to hold the reader to the end by art and illusion and not by proof and argument, and the moment he closes the cover and reflects he wakes up to their impossibility". They need no recommendation from the present writer.

Earthquake Prediction

In our issue for September last attention was drawn to the prediction by E. C. Thrupp, of Vancouver, that there would be a peak of seismic stress about November 1, with severe quakes at any time within ten days before or after that date. He now gives the following list of earthquakes as verification of his prediction: Oct. 14, Honduras; Oct. 18, North Italy; Oct. 20, Undersea near Java; Oct. 22 and 23, Alaska, Seward and Kenai Peninsula; Oct. 26, Tokio

and Yokohama (slight); Oct. 30, Los Angeles (slight); Nov. 2, East coast of Japan; Nov. 11, Pittsburgh, California (slight).

The present writer is not competent to express an opinion whether these reported quakes can rightly be considered a fulfilment of the forecast; this is left to more profound students of seismology. But there can be no general acceptance of the validity of Mr. Thrupp's predictions until he shows that they rest on solid scientific foundations. He is a reputable and experienced engineer and it is to be hoped that his promised book will clear up the situation.

C.A.C.

Let Us Have a Proper Symbol for Pluto

(Letter to the Editor)

Not long ago I received a letter from a prominent astronomer in the United States in which he said he was the author of the present symbol P[L] for Pluto and that it was acceptable to the Lowell Observatory, was placed in the iron railing in the new building at Harvard Observatory, was used by a former president of Harvard in his "Life of Percival Lowell", and is printed in the *American Ephemeris*, an official publication of the United States government. This weight of authority looks difficult to move in an effort to secure a truly appropriate and scientific symbol for this recent planetary discovery. But it is desirable to obtain a fitting symbol.

One may remark on some of the carelessly conceived symbols we already have, e.g., that for Leo, which is much like the one for the ascending node; those for Virgo and Scorpio, neither of which is truly symbolic; the curious combination of the symbols of the sun and Mars to take the place of Herschel's H for Uranus, and now the return to the "initial" method for Pluto. Percival Lowell called it X, let it be completed in a scientific way. Some persons thought the present symbol was a temporary suggestion awaiting a more carefully devised creation; and surely it should not remain if we can find something truly symbolic.

The change suggested is to keep the X and simply surround it with a circle. Letters are not scientific symbols except in algebra and chemistry and we never find monograms so used. The X has always stood for the unknown quantity and the circle may properly be added to indicate that astronomers have now surrounded this planet and included it in the solar family as well as within the circle of their ever-expanding knowledge.

May I submit the truly symbolic symbol of a multiplication sign within a circle for Pluto, and add that it can never be confused with ⊕ a plus sign within a circle, for the earth?

W. G. Colgrove.

London, Ont.

MEETINGS OF THE SOCIETY

AT VICTORIA

October 5, 1936.—The first regular meeting of the autumn session was held in the Y.W.C.A. lecture hall. President Tingley called the meeting to order at 8 p.m. The president reported that the Summer Course had been considered a success in its object of introducing the study of astronomy to a new group of about 25. The president announced a class in popular astronomy to be given by Miss Riddle at the Night School, also two radio talks on astronomy, one by Dr. W. E. Harper and one by the secretary.

The speaker of the evening, Dr. J. A. Pearce, spoke on "Eclipsing Stars". Algol, the prototype of eclipsing binaries, was known to the Greeks in the fifth century B.C. Its period of variability was discovered by John Goodricke, a deaf mute, in 1782. He later discovered the periods of Beta Lyrae, and Delta Cephei. Several types of variables were described, the Algol type being spherical in form with fairly large separation, while the Beta Lyrae type were ellipsoidal, their centre of gravity being closer and they revolved around each other in contact, inducing mutual tidal reaction on each other which caused perturbations in the orbital plane. The eclipse of the Beta Lyrae type was one of the riddles of the sky, the brighter and less massive star revolving inside the larger and darker companion. This type of star had surface temperatures ranging from 10,000 to 40,000 degrees. Another type, W Ursae Majoris, was described, being smaller in size than our sun but of great density and of a short period of from six to eight hours. Dr. Pearce, by numerous diagrams and slides, showed the great value of the study of binaries in determining the dimensions, masses and densities of the stars. Of the 13,000 known binary systems, astronomers had fairly accurate information concerning about only twenty-five and partial information regarding 200. Of this number the Dominion Astrophysical Observatory in Victoria, and the Mt. Wilson in California had contributed a generous two-thirds of the necessary study and work. Dr. Pearce stated that the most likely theory of the cause of double stars was that of binary fission or the dividing of a star in two. There were many examples of the different phases of fission.

GORDON SHAW, *Secretary.*

(Other minutes of the meetings held over.)

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